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THE PSYCHOLOGICAL REVIEW

DISCRIMINATION VS. LEARNING AND THE SCHEMATIC SOWBUG

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I. REASONS FOR MODIFICATION OF THE SCHEMATIC SOWBUG

In a recent paper in this journal (4) the writer presented some findings concerning 'vicarious trial and error' (VTE) as exhibited by rats in the learning of white-black and white-grey discriminations. These findings were that the rats 'VTE-ed' more in learning a white-black discrimination than in learning a white-grey discrimination. But in addition to these purely empirical results, the writer also presented a theoretical formula, called the 'schematic sowbug' to explain such results.

Further, it should now be stressed, as was not done in the original paper, that this theoretical formula—the schematic sowbug—was, as a matter of actual history, conceived first. In other words, the theory was conceived and the empirical findings were predicted from it before such findings were actually looked for. This fact of the temporal precedence of the theory to the findings is deserving of note. For a theory cannot but be felt to be well validated when it not only helps to unify and to make intelligible empirical facts already obtained, but when it also tends to lead to correct new predictions before the latter have ever been checked up on.[1]

[1] One must not, however, exaggerate this type of outcome too much. For it is evident that the number of correct findings which can thus be predicted beforehand from a theory depends not only upon the validity of that theory but also upon a number of other more or less irrelevant factors. It depends, for example, upon how many facts in the given area just happen to have already been gathered when the theory is conceived. Also it depends upon the degree to which the precise quantitative values can, at the given state of knowledge, be assigned to the parameters in the propositions and equations which constitute the theory. A theory may be entirely correct in its general outlines

At any rate, however significant or not it may be, it is the fact that my theory of the 'schematic sowbug' did correctly predict beforehand findings that were later found; and it is this circumstance which has led me to believe in the possible real fruitfulness of the theory and hence now to attempt certain further modifications and improvements.

In order, however, to understand better the full reasons for the new changes now to be introduced into the schematic sowbug let us recall again the experimental findings which were presented in the previous paper. The findings then were, as already indicated, that rats which were required to learn to discriminate between white and black doors tended to 'VTE' more than those which were required to learn to discriminate between white and grey doors. But this finding, it should now be emphasized, was somewhat surprising. For one would hardly have expected from mere common sense that, when two stimuli are easier to discriminate between, the organism would do more comparing, more VTE-ing, than when they are more difficult to discriminate between. How is such a contradiction with common sense to be explained?

The answer is suggested when one notes that the problem presented to the rats was not a threshold problem *per se* but, rather, a supra-threshold 'learning-what-to-do' problem. The rats did not know beforehand to what features of the stimulus-situation they were to pay attention. Their problem was not, as it is in the usual human analogy, that of discriminating at near threshold levels which door is brighter (already having learned and become thoroughly imbued with the *instruction* to choose the brighter) but rather that of 'discovering the instruction,' *i.e.*, of learning what to do. The rats had to learn that it was differences in visual characters and not in spatial positions, in odoriferousness, or in some other discriminanda-qualities, to which they were to pay attention. If one now thinks in terms of this corrected analogy, it seems reasonable (although I doubt if anyone would have thought of it beforehand without first having 'deduced' it from the sowbug or

and yet because of lack of knowledge concerning the precise values of some of its parameters be incapable, at the given time, of leading to any but relatively general or relatively puerile predictions.

from some other theory) that the animals should perhaps VTE more as a result of the greater brightness differences; for these bigger brightness differences would in common sense terms be more intriguing. Or, in schematic sowbug terms, the discriminanda-angle between the two stimulus-sources will be greater for the bigger white and black differences than for the smaller white and grey differences and hence lead to a greater imbalance between the 'orientation vectors' and thus to a stronger tendency to begin VTE-ing for the white-black discrimination than for the white-grey discrimination.

But how is this outcome to be made consonant with the other common sense expectation that when the instruction is already known and the differences between the two stimuli are near the threshold limits, VTE tends to be less, the bigger the stimulus-difference? To modify the schematic sowbug so as to make this latter outcome [2] also deducible from it is the purpose of the present paper.

II. Description of the Original Sowbug

By way of introduction to the new edition of the sowbug let me begin by briefly recapitulating the discussion and description of the original bug. First, it is to be recalled that the schematic sowbug was merely a physico-mathematical formula (not an actual animal)—a formula by means of which the effects of such *independent variables* as: 'degrees of difference between the two to-be-discriminated stimuli,' 'numbers of repetitions,' 'degrees of hunger,' 'width of barriers,' and the like, are (in a given discrimination set-up) to be theoretically combined so as to predict the actual resultant *dependent variables* of 'percentages of correct choices,' and relative 'amounts of VTE-ing' which will tend actually to occur.

Second, it is to be recalled that the underlying mechanism of the sowbug was assumed to be analogous to the mechanism assumed by Loeb in his doctrine of 'forced movements' in lower organisms with the addition of the distinction made by

[2] A new investigation, with human beings, has just been completed which indicates that this latter outcome of more VTE for the more difficult discrimination in a threshold problem does actually appear—at least in the case of human beings. And it is hoped that a somewhat similar experiment can later also be done with rats.

Blum between 'orientation propensities' and 'progression propensities.' (For references see 4.)

Third, it will be remembered that any given series of 'discriminanda' [3] as, for example, the series from white to black, were translated for the purposes of the sowbug into a series of angular directions about the bug's nose in one plane of the latter's 'life-space.' [4]

The above assumptions added together meant that when in a given discrimination set-up the rat (or any other organism) is presented with two stimulus sources lying in the same discriminanda-series, these two sources are to be represented as forming a certain angle with one another in front of the bug's nose. And the width of this angle is to be conceived as greater, the greater the degree of discriminable difference between these two stimulus sources. The sowbug concept posits a tendency for the animal to orient, in 'forced movement' fashion, towards both stimulus sources. Having once succeeded in orienting towards one of the two sources, he will, however, become somewhat satiated with respect to that one and then his orientation tendency towards the other will dominate. So he will next orient towards this latter. But in the meantime his orientation tendency for the first will have somewhat recovered, and so he will now orient back towards it. But this first tendency will again become satiated and so he will turn away again; and so on. The bug will oscillate back and forth. In short, he will VTE. And this VTE-ing will continue until, as a result of all these successive orientations, the orientation-needs towards both stimuli will have become relatively reduced—at least for the duration of a particular trial.[5]

Further, in so far as the organism, corresponding to which a sowbug is constructed, has any innate, or acquired, positive

[3] See Tolman (3), Chap. V.

[4] For the general concept of a 'life-space' see Lewin (1). I am really conceiving as many planes in the life-space of the sowbug as there are types of discriminanda-series to which the given animal can be sensitive. Thus, the complete sowbug would really be a multi-dimensional animal. But in most discrimination situations it is necessary to consider only one of these discriminanda planes.

[5] This notion that the successive orientations will eventually reduce both orientation-needs and that this will help to terminate VTE-ing on a given trial, was not emphasized in the previous paper.

or negative progression tendencies (called in the previous paper 'hypotheses') towards the one or the other of the two stimuli, the bug will also exhibit tendencies to progress positively (or negatively) towards the one or the other stimulus source. And these progression-hypotheses will strengthen with learning and eventually be strong enough to overcome all VTE-ing tendencies. When this occurs, the animal will jump at once towards the positive side, and keep away from the negative side.

Finally, in order to make the above clearer let me again present several of the figures shown in the original article with their accompanying descriptions. In Fig. 1 the different parts of the bug are labelled. One stimulus-source only is shown. A description of this figure may be quoted from the original article:

> It (the bug) is shown as lying at an angle to the parallel rays from the single stimulus source, say, white. It is bilaterally symmetrical. It has symmetrically placed pairs of motor appendages. These latter, however, are represented by vector arrows rather than as actual motor organs. The pair with small white rectangles on them, at the front just behind the head, are the orientation vectors, the pair with the plus marks on them at the rear are the progression vectors.
>
> The curve with small rectangles labelled 'orientation distribution' indicates the relative intensities of perceptual stimulation of the successive receptor points. These strengths of excitation are to be conceived as determined by the angles at which the rays hit plus the angular distances of the receptor points in question from the animal's median plane. The more perpendicularly a ray hits and the nearer the receptor point, which it hits, is to the nose (*i.e.*, the median plane), the stronger the excitation. When all the rays come (as in this case) from the left, the distribution is skewed and its mode lies to the left of the median plane. But when the rays come straight on, the orientation distribution becomes symmetrical. It is also to be assumed, following Loeb, that the right-hand motor appendages are activated by the left-hand receptors, and the left-hand motor appendages by the right-hand receptors. (I am assuming, that is, a positively taxic bug.) In other words, the area under the orientation distribution which is to the left of the median plane determines the length of the right-hand orientation vector, and the area under this curve which is to the right of the

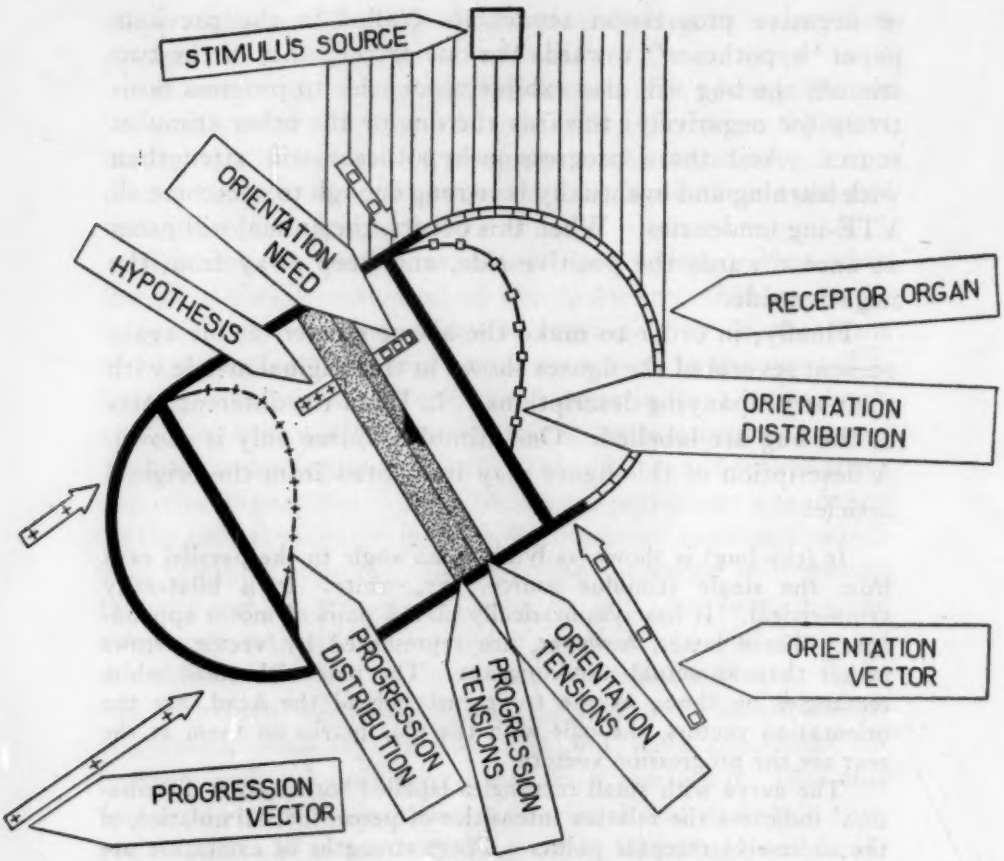

FIG. 1. The schematic sowbug. (After Tolman, 4, p. 325).

median plane determines the length of the left-hand orientation vector.

Further, it must be noted that although the shape of the orientation distribution is determined by the angular direction of the rays, its height is determined, rather, by a specific 'orientation need' relative to the given quality. This specific need is indicated by the little column with rectangles in it which is shown as rising up out of the upper stippled area, which latter represents the general orientation readiness or tension. This way of representing the tension or readiness as a stippled area inside the organism, the degree of stippling corresponding to the degree of the demand (derived in this case from hunger), I have adapted from Lewin (1). Further, it is also to be assumed that once the bug has looked

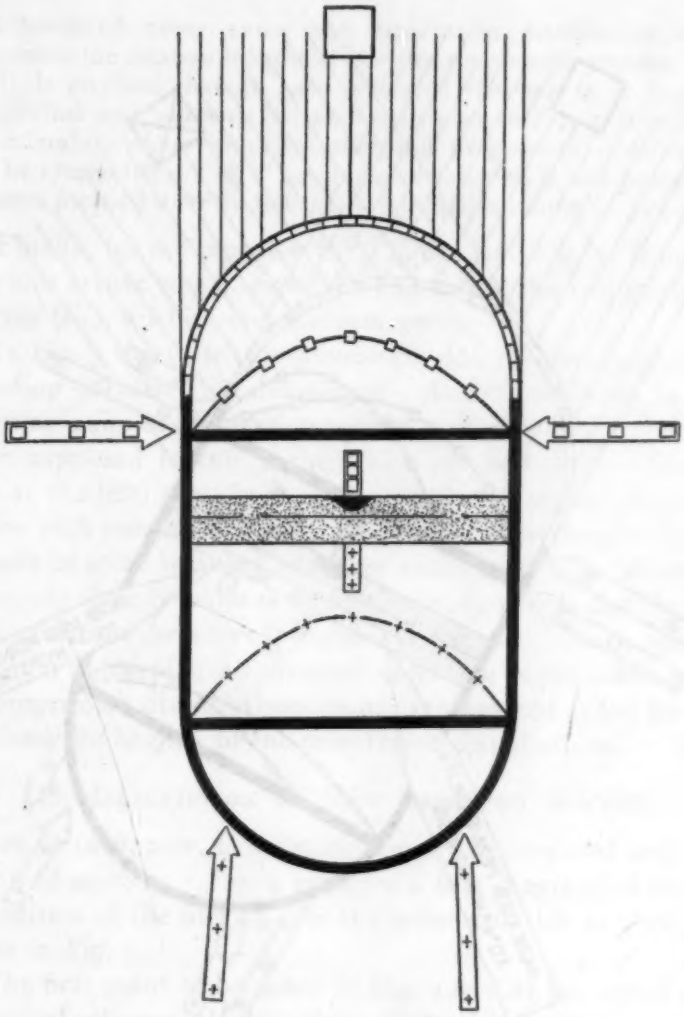

FIG. 2. Head-on orientation. (After Tolman, 4, p. 327).

directly at a given stimulus source the specific need relative to this specific quality and the corresponding resultant orientation distribution both temporarily sink.

The rear curve with the plus signs—the progression distribution—is to be conceived as following as to shape the orientation distribution. That is to say, its mode will shift following that of the orientation distribution when and if the animal turns. The height of this progression distribution is to be conceived, however, as

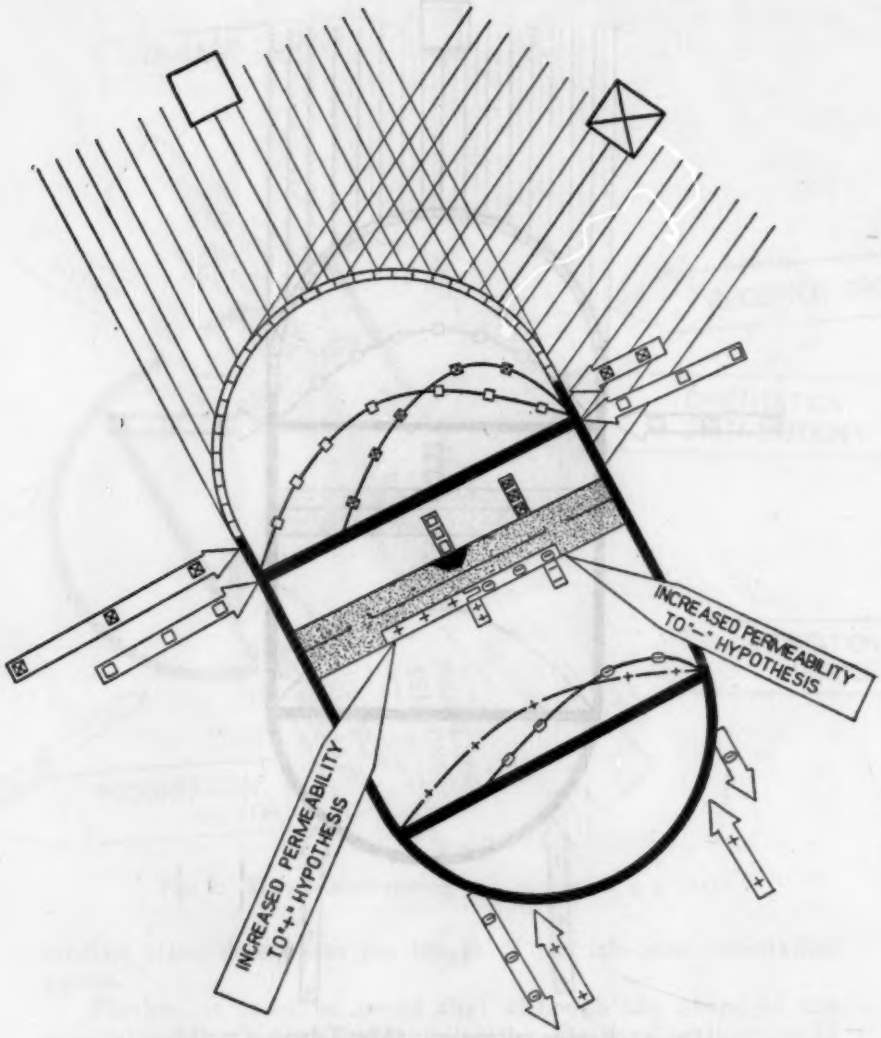

FIG. 3. Early in learning. (After Tolman, 4, p. 333).

determined by the strength of the specific hypothesis—represented by the lower column—to the effect that the given stimulus source is good. This hypothesis in its turn is a product of the general progression tension—represented by the lower stippled area—plus specific past experience relative to this specific type of stimulus source. It is such past experience which makes such a plus hypothesis ready to erupt. Note, further, that it is the right-hand

and left-hand areas under the progression distribution which determine the relative lengths of the two progression vectors.

It is obvious that as here indicated the bug is in unstable equilibrium and will turn left until, as shown in Fig. 2, it will face the stimulus, where its orientation and progression distributions will be symmetrical and, if nothing else happens, it will proceed to progress forward directly towards the stimulus source (4, p. 325f.).

Finally, let me repeat a third figure (see Fig. 3) from the previous article which shows the bug facing the two stimulus-sources (*e.g.*, a white and medium grey).

In Fig. 3 there are two orientation-distributions, one corresponding to each stimulus-source. And these result in two opposing pairs of orientation-vectors. Further, as a result of some supposed learning, the white stimulus-source (shown here at the left) already induces a positive progression distribution with resultant positive progression vectors; and also, as a result of some learning, the grey stimulus source (shown at the right) already induces a negative progression distribution with resultant negative progression vectors. Since only a modicum of learning is conceived as having as yet taken place the progression distributions are not represented as yet having obtained the heights of the orientation distributions.

III. Description of New Improved Sowbug

Let us turn, now, to a description of the proposed new and improved sowbug. Fig. 4 presents a first diagram of such a new edition of the bug and for the same situation as that just shown in Fig. 3.

The first point to be noted in Fig. 4 is that the upper pair of vertical columns which in the earlier edition of the bug were called 'orientation needs' are now designated as 'orientation readinesses.' 'Readiness' seems a more appropriate name than 'need,' for this factor is the result not only of need conditions but of learning facts as well. That is to say, as will be indicated below, we shall now assume in general that, when through instruction (or other means) the organism has *learned* which is the dimension of stimulus difference it is to pay attention to (brightnesses, sizes, shapes, or whatever), the

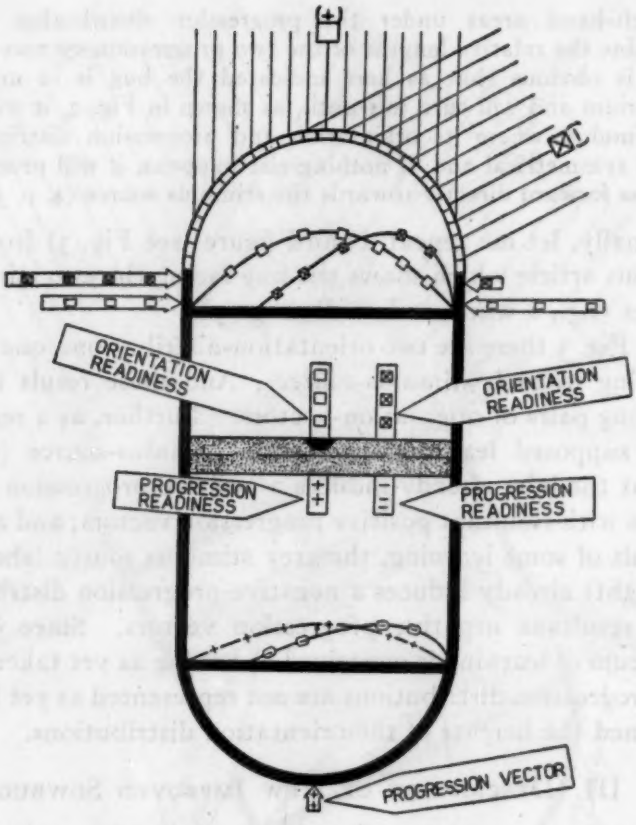

FIG. 4. New edition of sowbug early in learning.

greater are to be drawn the heights of both orientation-readinesses. A specific orientation readiness results from both need (*i.e.*, tension) and learning.

Look next at what in Fig. 4 are labelled as the progression readinesses. These were designated 'hypotheses' in the former diagrams. The term, progression readiness seems, however, a better one. It is less controversial and it has the advantage of being symmetrical to our other new term of orientation readiness.

Finally, look at the progression vector. Here is where the biggest innovation has been introduced. Instead of two pairs of progression vectors—the vectors of each pair corresponding

to the areas under the two halves of one of the two progression distributions—we have now but one, centrally placed, progression vector. And this is computed from the algebraic addition of the heights of the two progression distributions at the center point—that is to say, it is proportional to the algebraic sum, on the ordinate corresponding to the direction of the stimulus which is being faced, of the height of the positive progression distribution and the height of the negative distribution. (In the case as shown the positive distribution is higher.)[6] Further, in the final computation of this central progression vector, this algebraic sum of two ordinates is multiplied by a constant. This is to make it commensurable in size with orientation vectors which correspond to areas and not to simple linear distances. And I have chosen as this constant in the present diagrams a value equal to one-half the width of the bugs. The lengths of the progression vectors as drawn involve, then, this constant plus the use of the same scale for translating areas into linear distances used in the case of the orientation vectors.

So much for a general description of the new improved sowbug as shown in Fig. 4. Our next task is to try to use this new bug to answer our original question of how to deduce both greater VTE for an *easier discrimination* in the case of the problem of learning what-to-do as this was set for the rats and also greater VTE for a *more difficult discrimination* in the case of a pure discrimination problem as set for human beings.

IV. Application of the New Improved Sowbug to Explain VTE in Both Learning What-To-Do and in Pure Discrimination Problems

To show now how these two seemingly contradictory results can both be deduced from this new edition of the bug it will be helpful to present four values of the bug corresponding to the four conditions: (*A*) learning about to begin—easy discrimination; (*B*) learning about to begin—difficult discrimination;

[6] It is to be observed that our assumptions here become more similar to those of Spence (2) in his derivation of the response tendencies resulting from the two 'generalization' curves.

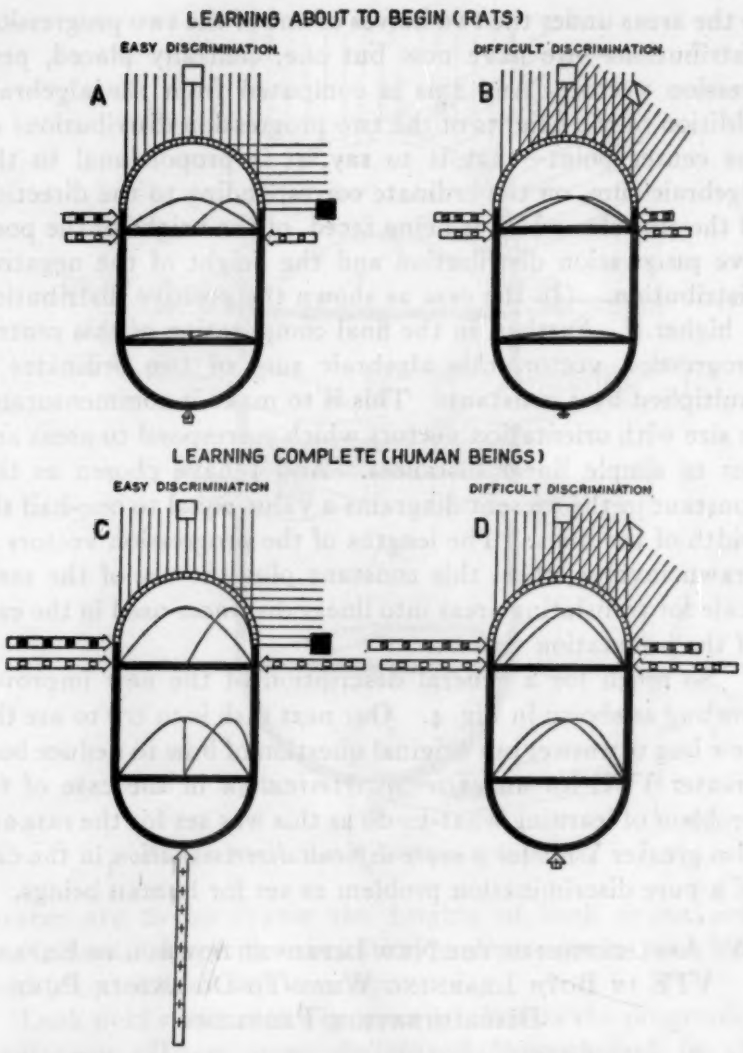

FIG. 5. Discrimination versus learning easy versus difficult discrimination.

(*C*) learning complete (by virtue of instructions)—easy discrimination; and (*D*) learning complete (by virtue of instructions)—difficult discrimination.

Figure 5 presents these four conditions. (*A*) and (*B*) at the top are for the learning problem (as usually presented to *rats*) in the usual visual discrimination set-up in which the

animals have to *learn* both what dimension it is they are to pay attention to and which stimulus is plus and which is negative in that dimension. (*C*) and (*D*) at the bottom are for the situations (as they are usually presented to *human beings*) in which the subjects, by virtue of instruction, already know clearly what it is they are to look for—brightness, sizes, or what not, and also which end of the dimension is to be plus and which negative.[7]

Consider, first, (*A*) and (*B*). The animals are just ready to begin to learn. It is evident that there is a much greater imbalance of the orientation vectors in (*A*), the case of the easy discrimination, than in (*B*), the case of the more difficult discrimination. And, since the progression vectors are practically nil in both cases, there will tend (simply as a result of the differences in the orientation vectors) to be more VTE-ing in (*A*) than in (*B*).[8]

Turn, next to (*C*) and (*D*), the case where learning has already taken place (by virtue of the instructions). First, it is to be noted that *another principle has been introduced here*—a principle which could not easily be shown in a single diagram such as Fig. 4. This new principle is that knowing what-to-do is to be conceived to increase not only the heights of the progression distributions but also the heights of the orientation distributions. In the preceding article (4) the effect of learning was depicted primarily as one of increasing the permeability of the membrane between the progression tension and the resulting hypotheses or, as we are now calling them (see Fig. 4), the resulting progression readinesses. Our new principle is that learning or knowing what-to-do also increases the permeability of the membrane between the orientation tension and the orientation readinesses.[9] In so far, then, as

[7] The insides of the bugs—*i.e.*, the tension-compartments and the orientation-readiness columns and the progression-readiness columns—have for simplicity's sake been omitted from these diagrams in Fig. 5.

[8] N.B. The assumptions and conclusions for these two cases are essentially the same as those of the previous paper.

[9] The question as to just how in detail the actual facts of learning—that is, the specific reward of the one stimulus and the punishment of the other—are to be conceived to operate to produce such general increased membrane permeabilities for the given dimension has not been stated. An adequate conceptualization of these details of the learning process will be a task in itself—a task to which I shall hope to address myself in a subsequent paper.

the animal has discovered (*or been told*) that it is the white-black dimension that he is to pay attention to, he has, we are now assuming, stronger orientation readinesses for all the stimuli in this white-black dimension. Looking again at Fig. 5, this new principle means that in (*D*)—the case of difficult discrimination after learning—the orientation vectors are somewhat longer than in the case of (*B*) depicted above it. But, since the progression vectors are very small in both cases, this means also that the VTE's in (*D*) will be greater than in (*B*).

Turn now, finally, to the direct comparison of (*C*) and (*D*). It will be seen at once that in (*C*), in spite of the big orientation vectors, there will be practically *no* VTE-ing because of the very great size of the progression vector. This latter will cause immediate choice of the correct stimulus before VTE-ing has had a chance to take place. Or, in short, where the stimulus difference is well above threshold there will be immediate and unhesitant choice of the correct stimulus (provided the animal already knows what to do). In case (*D*), on the other hand, as we have seen, there will be considerable VTE-ing due to the imbalance of the relatively large orientation vectors and to the smallness of the final progression vector. Or, in short, when in our human experiments the animal already knows what to do, our new form of the bug leads us to predict more VTE for the more difficult discrimination—*i.e.*, more VTE in (*D*) than in (*C*).

It may now be asked, however, how, since the progression vectors in cases (*A*), (*B*) and (*D*) are all practically nil, progression ever takes place. The answer is three fold.

In the first place, we are now assuming (see above footnote No. 5) that VTE-ing results in a successive reduction of both orientation readinesses to zero so that finally only the progression readinesses, however small, are left, and these by themselves will eventually cause progression.

Secondly, it must be remembered that in cases (*A*) and (*B*) (*i.e.*, the cases before the white-black dimension has been learned) the animal is nevertheless set for progression in other dimensions—say, the spatial dimension. He jumps in these cases after VTE-ing has been exhausted not by virtue of white

and black progression propensities which are the only ones shown in the diagram and which are as yet relatively nil but by virtue of strong spatial—*i.e.*, right or left—progression tendencies which he brings with him and which are still strong.

And, thirdly, in cases (*C*) and (*D*) in which the animal has already learned (or been told) to pay attention to the white-black dimension there can be assumed to be a new additional positive progression readiness set up for all the stimuli in this dimension. In other words, in case (*D*) there is to be assumed an additional progression vector which when VTE-ing is exhausted makes him ready to jump towards *either* white or black. In other words, even if he cannot discriminate at all, he will finally choose one stimulus or the other simply in order to obey the instructions.[10]

V. Summary

In conclusion, I wish to recapitulate and to emphasize the following points:

(1) The 'Schematic Sowbug' is a purely theoretical formula into which the values of such *independent variables* as 'degrees of physical difference between the two to-be-discriminated stimuli,' 'amounts of learning which have already taken place,' 'widths of barrier,' 'degrees of hunger,' [11] can be substituted in order to predict what should be the resulting *dependent variables* of 'percentages of errors' and accompanying 'amounts of VTE-ing.'

(2) A previous paper concerned itself with the effects of 'different degrees of difference between the two to-be-discriminated stimuli' upon 'errors' and 'VTE-ing' in rats when learning what-to-do had yet to take place. And the first edition of the schematic sowbug was devised primarily to predict the results under those conditions only. This original bug predicted that there should be more VTE-ing when the

[10] In a subsequent paper I hope to be able to indicate the operation of these factors in more detail.

[11] These last two have not, of course, as such been investigated to date. Experiments on them for both animals and human beings are, however, already completed or in progress.

discrimination was easy than when it was difficult. And this prediction was confirmed by an actual experiment.

(3) The present paper has concerned itself with the effects of different degrees of difference between the two to-be-discriminated stimuli upon errors and VTE-ing in human beings [12] after the problem—*i.e.*, the instruction as to what is to be done—has already been acquired. The results to be expected in this case were first predicted from common sense. These predicted results were that the smaller stimulus differences should cause the larger amounts of VTE. (Such results have been obtained in an actual experiment—carried out with human beings—to be reported in a separate paper.) A new edition of the sowbug has here been presented which will predict this second type of outcome also.

(4) Finally, I would like to emphasize again, for the sake of readers who may be temperamentally unreceptive, or positively antagonistic, to such a physiological-biological-geometrical scheme, that the schematic sowbug is merely my attempt at a predictive theory. If a more simple stimulus-response theory could also predict VTE's with equal or better accuracy, I for one, ought to be willing to abandon the sowbug.

[MS. received November 12, 1940]

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[12] A somewhat similar experiment could probably be carried out with rats in that after the rats had learned a number of supra threshold preferences for white as against different greys they would then be tested at near threshold limits.

THE TREND IN THEORIES OF ATTENTION

BY F. C. PASCHAL

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More than thirty years ago Titchener declared the doctrine of attention to be one of the three fundamental issues upon which any system of psychology must be based (37). Modern psychologies are far from being in agreement with this statement, as in none of them has it been a crucial problem. In fact most of them have ignored it, or have merely paused to pay their respects (or disrespects) in passing. The ebb of the influence of attention can be seen in the fact that in the past two decades many textbooks have omitted all reference to it. The biennial reviews of its literature in the *Psychological Bulletin* ceased ten years ago. But despite the criticisms that have been aimed at it, the attempts to read it out of the party, attention has had a hardiness that has enabled it to survive. Recently it has entered upon another phase of its cycle, a renewal of interest in its possibilities being in evidence. The fact that recent textbooks have been displaying substantial agreement as to the place of attention in the framework of psychology, characterized by an emphasis upon an aspect of behavior long neglected, has seemed to warrant a description of this trend.

The importance of attention in the rational psychology of the latter part of the past century was due to the closeness of its relationship to consciousness itself. The degree of consciousness, the intensity of consciousness, clearness of images or ideas, action of the will, the affective state (interest), the feeling of effort, the attitude of muscular tension, the preperceptive anticipation, exploration—all these were identified as being present in the attentive state and (as Pillsbury pointed out) at one time or another each was declared to be the essence of attention itself. Which was cause, which effect, which concomitant, and which mere by-product, varied from psychologist to psychologist. The analysis of consciousness

had been so exhaustive that Braunschweiger's statement to the effect that nothing new would be added to attention which had not already *in nuce* been said, still holds true. As the roots of psychology lay in physiology, much of the interest centered about the neural correlates, and physiological theories were almost as numerous as were psychological theories.[1]

The most popular of the psychological theories in the latter part of the period was of course that which identified attention with clearness in consciousness. This interpretation was first set forth early in the eighteenth century by Malebranch (**32**), who is credited with having first devoted a chapter to the subject. It reached its apex in the work of Titchener, whose name is so closely associated in our minds with the study of attention.

When the Functionalist School appeared, attention was assigned the role of selectivity. According to William James:

> It is the taking possession by the mind, in clear and vivid form, of one out of what seems several simultaneously possible objects or trains of thought. Focalization, concentration of consciousness are its essence. It implies withdrawal from some things in order to deal effectively with others, . . . (**19**, p. 403).

While the Functionalists made clear their position that in the plan of human adaptation, the selection of stimuli operates by means of that process called attention, they were not in agreement as to how this selection takes place. Angell attributed it to an action of the will. Dewey called it an activity of the mind itself, an active association. James described it in terms of anticipatory adjustment.

> And two physiological processes, of which we have got a glimpse, immediately suggest themselves as possibly forming in combination a complete reply. I mean:
>
> (1) *The accommodation or adjustment of the sensory organs;* and
>
> (2) *The anticipatory preparation from within of the ideational centres concerned with the object to which the attention is paid* (**19**, p. 434).

[1] The history of attention from the standpoint of the physiological theories was written by Braunschweiger (**4**) in 1898 and the well-known comprehensive survey of the whole field of attention, ten years later by Pillsbury (**30**).

Speaking of the second of these processes, he says:

The effort to attend to the marginal region of the picture consists in nothing more or less than the effort to form as clear an idea as is possible of what is there portrayed (19, p. 438).

Evidence from Wundt's and Exner's experiments supports his position that the preparation consists of the anticipatory imagination of what the impressions or reactions are to be, and he suggests that Lewes' term *preperception* seems the best possible designation.

It is rather strange that as general as has been the acceptance of the idea that the function of attention is selection, and as often as James is quoted for his recognition of the sustaining contribution of the neuro-muscular system, this description of the attention process should have been so completely ignored. The only discussion of it the writer has found is in a recent article by Mowrer (25). Even Marzi (23), whose experimental program is based upon the selective theory, and who begins his discussion by quoting James, fails to make any reference to it.

Since that time most of the American textbooks have called attention a selective process, but the descriptive material has generally been influenced by the Structuralist emphasis upon the phenomenal aspect. This fact is to be attributed to the influence of Titchener, in whose laboratory a comprehensive experimental program continued for some time, while there was no corresponding research coming from Functionalist quarters. In France and Italy the selective interpretation was even more widely emphasized than here. This emphasis is probably to be attributed to the influence of Binet, who was himself a follower of James. Attention is an adaptive process, said Binet (2), and is essentially the direction of thought. How that direction takes place he does not attempt to say; it is an observed fact that it does.

With the appearance of Behaviorism, interest in attention began to decline. The cause of this regression is to be found in the swing in psychological methodology from an emphasis upon the phenomenal aspects of experience to the behavioral

aspects, and the consequent tendency to reject terminology that had acquired a mentalistic tinge. Watson ignored attention. Bekhterev (1) reduced sensory attention to the reflex adjustment to the incoming stimulus and ideational attention (concentration) to implicit speech.

Holt, as spokesman for the Behaviorists, since it was necessary for some one to deal with this persistent problem, established the pattern of their reaction, by declaring that as the faculty of attention had been analyzed out, separating all the factors that belonged elsewhere, there remained only the core of clearness. Seeking that which in behavior would correspond to attention in cognition, he finds it in "the process whereby the *body* assumes and exercises an adjustment or motor set such that its activities are some function of an object; are focused on the object" (17, p. 178).

One of the best subsequent statements of the objective position is that of Dashiell (7), who under the heading, 'Attending as a Form of Posturing,' says:

> *When a person takes up an attitude that will facilitate his response to some particular stimulus or stimuli, that attitude goes by the name of attending or attention* (7, p. 285).

The act of attending includes (1) adjusting the receptive mechanisms, (2) more widely distributed postural changes, (3) respiratory and circular functions, and (4) diffuse muscular strains.

Now if attention is but a name for a set of strains plus some organic sensations, even though response may be facilitated, then it can have but a minor place in the psychological framework. Again the question arises as to whether these reactions are the essence of attention, or whether they are the physiological concomitants or consequents of that mental act which anticipates the stimulus. Is the process of selection adequately described in terms of the adjustment of the receptive mechanism?

While systematic psychologists were questioning the validity of the term 'attention,' criticizing it as meaningless, unnecessary and mentalistic, it continued to occupy a salient

position in the various branches of applied psychology. The workers in these fields were undisturbed by the differences in views as to the essential nature of attention but contented themselves with the common sense interpretation. Psychiatrists, psychotechnicians and educational psychologists were faced with the necessity of measuring attentional variations, or reactions whose major antecedents were attentional. Their studies have constituted almost the whole of the experimental literature of the past decade.

In the industrial and personnel fields, it is a basic assumption that there is a trait (or traits) identified with the ability to pay attention and facility in shifting attention which is essential to success in most operations. In consequence, batteries of occupational and aptitude tests regularly include tests of attention. German and Russian studies in job analysis and selection are the source of some of the more comprehensive reports on the place of attention and unique tests for its measurement. The more frequent problems in psychotechnology are those of types of attention (the question of diffused versus concentrated attention), temporal span, spatial span, distractibility, including fatigue effects, and proneness to accident. These problems have been particularly prominent in the literature of the new field of traffic research, in both its major phases, *i.e.*, factors determining operating ability and personal factors affecting safety of operation.

In the field of mental tests, the degree of concentration, freedom from perseveration and range of attention have been tested if not directly, then by an emphasis upon tests demanding the maintenance of a high level of efficiency. The discussions relative to the speed factor in the test score bear upon this point. A number of efforts have been made both here and abroad to develop batteries of tests for the measurement of attention. The peculiarly close relationship of attention to intelligence has been emphasized by workers in the latter field from the time of Binet. As in the case of consciousness, the functional interdependence is such that a reliable measure of attention would seem one of the best

indicators of the degree of intelligence. This view is shown by Richardson, who said:

> Or we may even carry our analysis a step further and search for a general factor which enters into all the processes which are ordinarily described as 'intelligent.' To the writer it seems that attention constitutes such a factor, a belief which Binet himself appears to have held. The elementary intelligent processes—discrimination, comparison, analysis, synthesis—are all functions of attention. Intelligence would then be defined as the functional efficiency of attention (31, p. 13).

With this interpretation, Spearman also is in essential agreement.

Psychopathology would find considerable difficulty in dispensing with this concept. Attention is looked upon as one of the more frequent and clinically significant correlates of neural disorder. Psychiatrists and clinical psychologists have accepted as an operating definition, the control of the direction of response. Sollier had long before declared that it was just this lack of control that was responsible for the inability to make adaptations which we denote as feeblemindedness. Binet had defined the levels of feeblemindedness in terms of attentional reactions. These statements are not inconsistent with current views. Consequently adequate measures of attention would be most helpful to workers in the field of mental deficiency.

There are distinctive attentional symptoms in the psychopathic states. Among those having diagnostic values are the blocking of attention in manic-depressive insanity, the heightened threshold in dementia praecox, the low span in hysteria, the inability to concentrate in mania and the excessive fixation in paresis. In the diagnosis of brain injuries, lesions and tumors, attentional deviations play a definite part.

The activity in these various fields has helped to keep interest alive. Systematic psychology may deny responsibility for the needs of its more practical offspring; it can hardly overlook the evidences they offer of some pattern of reaction that is a determinant of human efficiency. We are

interested in knowing whether there is a unique series of behavioral events upon which this differential performance is based. If such is found, more adequate methods of measurement may be expected to follow more accurate description of the process.

Before attention could readjust itself to the status to which the behaviorist revolution had reduced it, another whole area, that of the postural determinants of reaction was preempted by the newly developing field of *mental set.* The story of attention would not be complete without a sketch of the development of this new rubric. Mental set, or preparatory set, has a more extensive, recent literature than has attention, but we shall note only the significant stages that show this relationship.

Its antecedents are to be found in Ach's *einstellung* and Watt's *aufgabe*, which grew out of the problems of the preparatory interval in reaction time and the relating function in weight lifting. In its subsequent history, it appeared chiefly in the role of the directive factor in controlled association. The neural explanation was that cortical tracts, still partially active in consequence of a previous stimulation, respond more readily to any appropriate stimulus.

From set, which had been loosely applied to a variety of attitudes, Thorndike coined the phrase 'mental set,' to denote the idea in mind at the moment as a determinant of attention. In this sense it appears in his *Elements of Psychology*, in 1905. Its present application is to be attributed to Woodworth. In his *Dynamic Psychology* (44), surveying the drives and preparatory reactions, he said:

> The *mental set*, or intention of performing a certain operation or solving a certain problem, is a drive, reinforcing certain associative connections and inhibiting others, and thus exerting a selective influence (43, p. 124).

In view of his sponsorship, his subsequent expressions are of interest.

In the 1921 edition of the textbook (44), Woodworth treated attention as the focusing or exploratory phase of perception, while under another heading, controlled associa-

tion, he described mental set as selecting from the previously formed associations the one fitting the present task. In the 1934 edition, he treated attention in much the same way, though with more emphasis upon selectivity, while in his new chapter on 'Motivation,' mental set gives way to *preparatory set* or adjustment, more neural than muscular, consisting in going as far as possible toward the execution of the act without actually releasing it. In his 1940 edition, he brings set, selectivity and attention together. The statement just quoted is not retained. As in his *Experimental Psychology*, he devotes his efforts more to goal-set and task-set, to which he had previously made outstanding contributions. These sets are much more general, having to do with the orientation of the organism as a factor in selection, in contrast with the set or preparation for the reception of a specific stimulus.

Jersild (**20**) made a study of the shift of mental set—a restatement of the old problem of distributive attention. Using tests of form and color naming, multiplication and addition, and controlled association, he concluded that there is no dichotomy between set and shift but that "the ability that makes for efficiency in the separate tasks also makes for efficiency in the shift performances" (**20**, p. 79). He acknowledged the attention history of the experiment but preferred the term *set* as the "faculty of attention is misleading."

In the textbook already mentioned, Dashiell (**7**), 1928, brought set and attention into juxtaposition as the two types of postural response. He identified set as *einstellung* and interpreted it in terms of tonic reactions which predispose to the continuance of an activity and influence new subsequent activities. Just what the relationship between set and attention may be is here not quite clear. He makes this relationship clear, however, in his later book (**8**), in which he redefines attention as the assumption of a set. As to what the individual is doing when he assumes a set, Dashiell states that he is adjusting receptive mechanisms primarily, with the accompanying diffuse postural and circulatory changes completing the picture. In other words, when we are attending,

we are performing one active stage of the set, adjusting the receptors. Here we have a peripheral theory of set.

Somewhat similar is the interpretation of Fearing who implies some reflexive factor differentiating controlled and uncontrolled attention, and then suggests that "tonus *is* attention expressed in neuromuscular terms" (**12**, p. 231).

Freeman (**13**) holds a view similar to that of Dashiell, having defined set as the manifestation of the proprioceptive-tonic influence upon the exteroceptive-phasic reactions, thus providing for the perseveration of the neuromuscular response.

Hoisington (**16**), who also takes a postural view of attention, made the relationship a little more specific by stating that attention is a set for the sensory processes, while set itself applies to the motor processes.

In a recent article Mowrer (**26**) and others have given evidence pointing to the *central locus* of set, as opposed to the views of Freeman and Dashiell, who favor its peripheral localization. They have produced set phenomena under conditions that seem to them to preclude the presence of a corresponding motor set or tonic adjustment.

The proponents of mental set have quite generally been inclined to assume that the phenomena of attention are adequately provided for through this approach. They have cited the disagreements among the older theories, the inadequacy of many of them, the identity of the events considered by each, and particularly the mentalistic connotations of attention. But that there has been no general acceptance of the identity of set and attention is evidenced by the fact that they are given separate bibliographical classifications and by the total absence of cross-references, save for the common use of Woodrow's classic experiment.

We stated on an earlier page that two distinct interpretations of attention had developed in parallel fashion. While members of the radical party were adhering to the *set* platform, the conservatives (except for the remaining members of the structuralist wing) were thinking in terms of selectivity and were therefore turning toward the preparatory adjustment. While there was universal agreement as to the selective

function of both set and attention, not all theories had been pointed towards the explanation of the selective process. The second direction of development started with a return to the theory of William James.

The physiological theories of attention in the post-war period were of two types, those denoting attention as the mental aspect of the neural processes of facilitation and inhibition, and those that looked upon the neural substrata as a state of partial activation of cortical areas. The latter has already been identified as the central theory of set. But as psychology was now becoming more interested in the description of reaction systems, neither of these theories was of vital concern.

Kantor was the first to treat the subject exhaustively from the standpoint of the mode of behavior exhibited.

> Attention reactions are then the indispensable preliminary reactions to all psychological conduct. Only after a stimulus has been actualized can the person proceed with the performance of whatever response is elicited by the specific stimulus. In this sense attention reactions are not only preliminary but they are definite preparatory actions. They function to prepare the individual for whatever type of action is to follow. In more complex behavior situations attentional reactions are more than preparatory, they are in a genuine sense anticipatory . . . (**22**, p. 217).

Here we have attention defined as a reaction. It is more than just a part of the perceptual reaction; it determines whether the organism will perform a given reaction at all. Included are both affective and cognitive components, though the latter he reduces to a "momentary appreciation of the presence of a stimulus object" (**22**, p. 222). His actions which 'actualize stimuli' range from simple postural changes to the behavior complexes that serve to sustain attention in reading or thinking. In general, Kantor may be said to have restated James' position in terms of a stimulus-response psychology.

The textbooks of the period immediately following generally interpreted attention as a preliminary act of adjustment. Some carried the idea no further, others attempted to describe

the *modus operandi.* Gault and Howard say "It is characterized by the gathering of all the forces of our being into a single organ of action. It is 'organismic' response and adjustment" (**14**, p. 152). They suggest that the heightened innervation of the expected pathway is induced reflexively, thus determining to which stimulus the organism will react. Cole (**6**), in a text that is distinctly mentalistic, gives a similar statement as to its nature and follows with just such a discussion of the anticipatory images as we have seen in James.[2]

Piéron calls attention "a pre-perceptive reaction of expectancy" (**29**, p. 65 ff.), which consists of (1) movements and attitudes of exploration, (2) muscular readiness for response and (3) a general excitation setting off the affective coefficient. In his comprehensive survey of attention a short time later, he emphasized the energy release. "Attention represents an elective orientation of activity with liberation of considerable energy in the service of the predominant activity" (**29**, p. 75). We find in the writings of Piéron not only a more definitive statement of the anticipatory act, but also the beginnings of a new emphasis upon the routing of energy, which is suggestive of a neglected aspect of behavior.

There followed a short period in which the prevalent interpretation (in new texts) was that attention is the name for the total adjustment of the organism (Dockeray, 1932; Franz and Gordon, 1933; Griffiths, 1935; Murphy, 1935). This interpretation constituted a reversion from the view that it is a specific reaction. In general, this tendency may be attributed to an effort to adapt the *set* theory or to a revolt against the still common sensory adaptation theory. Murphy, contending that attention is an adjustment of the whole organism rather than merely a selection by sense organs, cites the fact that a frequent characteristic in cases of the mentally disordered is the "distractibility of the patient, the fact that he is unable to maintain any kind of *internal steering*

[2] If this were intended to be a history of attention from 1908, several interesting theories could not be omitted. A critical consideration of the several psychological systems in their attitudes toward attention would also be called for. The intent, however, is merely to trace the steps through which we have reached the current emphasis upon the character of the anticipatory reaction.

process, to hold attention on any one thing" (27, p. 224). (Italics are the author's.) As to how selection occurs, he suggests that "perhaps the thing attended to is the thing which arouses the most concentrated brain activity," but he does not hazard a suggestion as to how that increased brain activity is initiated.[3]

In his *Psychology Down the Ages* Spearman (34) takes up in its turn the story of attention. Having shown the conflicting views in the early history of psychology, but including no reference to contributions of the past quarter of a century, he reiterates the criticisms that, like intelligence, attention is altogether too indefinite a concept and that it has been too often used as an explanatory principle. Having traced the development of the word from its origin on into psychology, he declares that if it has any meaning, it can only be that of directing mental energy. He remarks in closing that despite the diversity of its definitions, it has been uniformly applied and that its survival may be due to its superior fitness.

The textbooks of the last few years have without exception, we believe, included a treatment of attention. And all of them have treated it, however expressed, as an anticipatory act. As to the character of that act, however, there is some confusion. This is undoubtedly due to the current requirement that such definition shall be in operational terms.

The idea of the direction of mental energy is combined with that of the preperceptive nature of attention by Stern:

> Attention is that personal state which constitutes the immediate prerequisite of a personal performance. The essential characteristics of attention are the clarification of the goal in consciousness and the concentration of force upon clarifying and attaining it (35, p. 472).

[3] It is, of course, hardly fair to interpret any man's theories entirely from his presentation of a topic in an elementary text. Yet we can identify the direction of his thinking, and there is so little else to go by. The lack of definitive expositions is responsible for the fact that changes have been almost imperceptible, laying the writer open to the charge that he may have read too much or too little into the specific quotations he has selected as illustrative of the stages in the transformation. Only those quotations are used, naturally, that represent their author's thinking. Many interesting and suggestive ones are rejected because their novelty is displayed only outside their setting.

A number of those persons who have endeavored to explain the nature of the act have indicated that it is due to *set* (Varnum, 1938; Guilford, 1940). Varnum is particularly definite in his description. "In paying attention, we are already doing the thing but we are doing it at the implicit level" (38, p. 196). His general treatment suggests that he identifies this activity with *set*.

In their first book, Boring, Langfeld and Weld, 1935, make no reference to attention but in their more recent book (3), 1939, in a chapter written by Feldman and Weld on 'Attention: the Control of Perception,' they say:

> Obviously, then, stimuli do not always induce perception. In fact, stimulation becomes effective for perception only (1) when it represents a change from the established level of stimulation or (2) when the individual is already prepared or primed to perceive (3, p. 414).

They suggest that priming is a searching, anticipating, concentrating, induced by the situation. That there is a stage of preparation immediately preceding perception is recognized, but no discussion of its structure or mechanism is included.

Whether an 'elective orientation of activity,' an 'internal steering process,' 'priming' or 'clarification of goal,' the implication is that attention is an active process, purposive and anticipatory. As such, no survey of the adjustmental processes is complete that omits it from consideration.

In resumé, we may state that theories of attention, having departed from the cognitive definition, took a teleological direction, identifying attention with the selective aspect of selective adaptation. There is no attention; it is but a name for the fact of selection. The next step was its identification with preparatory reaction, namely, a step from a teleological to a reactional theory. Attention is anticipatory in the sense that it is exploratory or forward-looking. It next became a more definite preperceptive preparation. Then entered a second factor, that of the control of energy, which brought it into conformity with the psychotechnical and non-technical uses of the term. There followed a trend toward specificity of the preparatory act. Its essential

nature is given an explanation in terms of preparatory set. These phases cannot be strictly delineated, of course, but they do serve to indicate the course of a trend that has not heretofore been made explicit. The next step is logically in the direction of operational description.

Two theories have come into the foreground, then, out of the thinking of the past quarter of a century. The one, the mental set theory, looks upon attention as a vague, diffused term for phenomena which are adequately covered by mental set, as defined in terms of the tonic-postural augmentation of the stimulus impulse, with or without the end-organ adjustments, and with or without a reflexive sensitizing of the receptor cells. The other theory, a selective theory, sees in attention a preparatory act of adjustment which serves to pave the way for a particular stimulus. These views are not mutually exclusive. The only conflict between the two need be the rivalry resulting from differences in emphasis. Dashiell has said that psychology has neglected the influence of set; it has also underestimated the importance of the entire selective process.

Attention operates through the lowering of the threshold. Not all lowerers of threshold, however, fall within the field of attention. Among those that do not are the permanent conditions arising out of the history of the organism which have in the past often been listed as conditions of attention, but which more properly are conditions of perception. The situation-set and task-set are also of this class. On the other hand there are situations in which the threshold is not lowered and which have been classed as attentional, but which would not be judged as such, according to the preparatory reaction theory. An instance of this is the initiation of a reaction by a change in stimulus conditions without the intervention of a preparatory stage. The fact that a stimulus breaks over an unlowered threshold implies attention only if it is defined in terms of degree of consciousness.

The fact that when the organism reacts effectively to one situation it does not react simultaneously to other situations, has been attributed to an inhibiting action of attention.

Dr. Johnson (21) and the writer, as well as others, have shown that under oxygen deprivation the psychomotor efficiency in one task is increased along with, or by a process of, the constriction of the field of reaction—the raising of other thresholds. As Dashiell has pointed out (9), it is not the continuation that calls for explanation; no attention function need be called upon unless to explain the breaking over from the constricted field. The impulse finds its way across the lower threshold; an attentional reaction operates to induce an immediate change in ratio.

In line with the development of the theory, we suggest that the act of attention is an *implicit anticipatory reaction* approximating identity with the reaction that is to follow. The degrees of attention correspond to the degrees of identity between the implicit anticipating and the subsequent overt reactions. Those factors of human efficiency commonly attributed to the power of attention are to be interpreted in terms of the arousing and maintaining of implicit anticipatory reactions. These reactions may range from the exploratory movements, directed adjustments of the receptor organs, on the one hand, to imaged duplicates of the expected stimulus on the other. The experiments in Witmer's (40) chapter on Preperception give excellent illustrations of this. One of the best is his use of varying instructions in advance of the display of the 'reversible staircase' diagram—"Now I am going to show you a Japanese screen," "Now a row of pup tents." The speed of perception varies with the degree of correspondence of the aroused image.

One of the objections to the treatment of attention in most textbooks is its confinement to the field of sensory perception. No theory may be considered adequate which does not account for ideational attention. Here again, implicit reactions provide for the maintenance and augmentation of a given pattern. In large part this provision is through the medium of implicit speech. This fact is particularly evident in efforts to read or to think in the midst of distracting influences. Just as in Morgan's distraction experiment, resort is made to more intense motor efforts, passing over

even into overt behavior, to provide the revivifying flow needed to maintain efficiency in the task.

That implicit movements occur in these situations can be attested by all of us. That they always occur is perhaps an allowable generalization from the study of Jacobsen, who concluded that

> During imagination, . . . recollection and concrete or abstract thinking involving . . . words or numbers, muscular contractions characteristically appear as specific components of the physiologic process of mental activity (18, p. 209).

Along with attention, mental set constitutes an immediate determinant of reaction. Attention differs from mental set in that it is an active, purposive process. Mental set is the tonic aspect; attention the phasic. Mental set refers to an antecedent state which may or may not have been established by an act of attention, or sustained by repeated acts of attention (concentration). They merge, the one (attention) into the other (mental set); hence the problems and methods of attack overlap. Together they constitute the preparatory state of adjustment that determines to which of the simultaneously presented stimuli the organism will respond.

In the light of this definition of attention let us now re-examine both the efforts to isolate an attention trait and the methods of the measurement of attention. Spearman (33), using McQueen's tests for diffused and concentrative attention, to which he applied the tetrad technique, found that intensive and extensive cognition correlate to the same amount with 'g,' from which he concludes that neither is a group factor, but rather they are dimensions of 'g.' Thurstone (36) reports nothing of the nature of an attention factor among his primary traits. This may be due to the fact that the tests were not chosen with the factor of attention in mind, and an inspection of his tests would lead one to expect that any attention factor is carried within the P factor. Woodrow (42) included tests that Philip had prepared specifically for the measurement of attention and found his fourth factor to be of a type he elected to denote 'attention.'

Morris (24) applied the methods of factor analysis to a battery of performance tests and found three factors to be sufficient. In only one test, the Witmer Cylinder test, was there a large residual. It has long been this writer's contention that the peculiar merit of this test lies in its demand upon distribution of attention. Easley (11) sought evidence of a relationship between attention and 'g,' using a battery of so-called attention tests along with several intelligence tests. His results were negative.

The methods commonly used in the measurement of attention are (1) simultaneous disparate activities, (2) alternating disparate activities, (3) distraction, (4) rate of discrimination, (5) rate of work (maintenance of efficiency) and (6) variations in limen.[4] It has commonly been said that attention, like intelligence, cannot be measured directly, only indirectly. Unlike intelligence and achievement measurements, however, no adequate criteria are set up as checks and no validity coefficients are offered in evidence. Nor are the low coefficients of correlation between the so-called attention tests viewed as indicative of their unreliability. Tests of perceptual or perceptuo-motor reactions are commonly chosen in which known traits, such as the eduction of relations, and previous experience are eliminated. The obtained scores are said to be measures of efficiency, or the ability to maintain efficiency and the assumption is that the prime variable is the attention factor. Hence efficiency tests are indirectly tests of attention.

We will accept the premises subject to the condition that the test shall be so constructed as to emphasize the anticipatory factor. In the light of this condition, the negative results obtained in many attention studies are understandable.

Many of the studies in which attention has been measured have been open to the criticism that certain factors have not

[4] The classifications here given are intended to cover the methods commonly used in the differential measurement of attention. We hesitate to criticize experimental techniques apart from their individual settings, as experimental methodology provides for checks that are lacking in differential measurement.

The earlier tests have been reported by Whipple (39), Geissler (15) reviewed the experimental work up to 1908 and Brown (5) presents a continuation in chronological form up to 1926. Mowrer (25) has classified the types of measurement of *set* in 1940.

been kept under control. These factors are (1) the complexity of set, (2) the reduction to habit and (3) the energy level. The first three methods named above are particularly subject to such misuse. When a method of disparate activities is used, for instance, it is assumed that the only variable is the speed of focalization.[5] But if performance in one of the component activities changes with practice, a source of error has been introduced. If during the course of the experiment the reactor develops a single complex set which provides an adjustment to both activities, then speed of focalization is no longer being measured.

The method of distraction has been a useful experimental method, but as yet it remains to be adapted to measurement of a trait. The one productive direction has been that of Woodrow's (41) experiment, in which reaction times with variable preparatory intervals were used.

In the light of the studies of Thurstone and of Woodrow, it may be questioned whether the method of discrimination rate does not contain too large a perceptual factor, at least in the forms in which it has commonly been used. It offers the greatest promise of any of the methods if tests are constructed in the light of the condition we have laid down.

The fifth method, that of rate of work, has been particularly misused. In the cancellation of A's, as an example, a distinct learning curve may be seen. The task-set will account for some improvement in almost any series, but the repetition of a stimulus-response bond must be avoided if the efficiency of response is assumed to be a measure of attention. The criticism that the energy level is not maintained evenly is a difficult one to answer in any test situation, but more especially in attention tests, particularly of this type. About the only safeguard is to keep the length of the series within moderate bounds, in the hope that loss of interest will not occur or fatigue set in before the end of the test.

[5] We have omitted from consideration the problem of perseveration, which has a large literature of its own. If perseveration is a unitary trait, its relationship to attention becomes a special problem. If it is not an independent variable, it may be expected to fit into our design as a factor in or as the negative aspect of speed of focalization.

But particularly we should say that when using this method, the test must be so designed as to require a continuous series of anticipatory reactions to maintain the speed of reaction.

The reference to Thurstone's study reminds us that the question of 'form' versus 'content' appears as a problem in attention. Dwelshauvers (10) argued that there are 'attentions' just as there are memories, but no 'attention.' Dashiell (9) made a similar suggestion in regard to *set*:

> Set, then, is in part a character of the organization of particular 'behaviors' rather than something in addition to them (9, p. 299).

The results derived by Thurstone lead him to the conclusion that individual differences are delineated by primary areas, that in the linguistic or in visual form the ratings in the several functions may be expected to be relatively uniform. Over against this is the proposition that the differential ability to make anticipatory responses is a function of 'g.' It does not follow, however, that this ability would be equally operative in all the primary areas.

There remains the question of terminology. Has attention had such a variety of meanings as to destroy its usefulness? Would another term carry our implications more effectively? *Selectivity*, *mental set* and *anticipatory adjustment* alone are free from connotations that disqualify them. The extension of the term *set* to cover the operations here defined might subject a useful term to the very criticism that has been leveled against attention. Purposive selectivity is accomplished through preparatory adjustment which includes not only the act we have called attention, but also the support of the established set. Since, in many instances, selection is brought about through the operation of set in the absence of any definite anticipatory adjustment, attention is not synonymous with *selectivity*. Perhaps it would be more exact and more satisfactory to call it *anticipatory adjustment*. While that term has been used in a broader sense by Dodge, it at least is a term that is not preempted. However, the objections to the use of the term attention do not for the most part seem valid. Some were based on the indefiniteness

of the concept. We have endeavored to show that to be less true today. Some would discard the fowl with the feathers. Some were men of straw. The most valid objection is to the use of the noun rather than the verb form. It is an act, not a state. Certainly in the sense in which the term appears here, it meets the condition set down by Spearman, that it refer to the direction of mental energy. As such it conforms to the practice in the various fields of applied psychology. It is consistent with common usage as seen in such phrases as 'to turn attention to' or 'attention is called to.' Literary usage is at least suggestive. The final test, however, will be of the concept and not of its label. It is to be hoped that the label does not mislead or distract. It remains to be seen whether such a redefinition will better serve the needs of psychotechnicians and psychiatrists and will stimulate more effective research than have some of the theories of the past.

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THE NEED FOR A PHENOMENOLOGICAL SYSTEM OF PSYCHOLOGY

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I. Confusion Due to Data From Two Different Frames of Reference

No visitor to a meeting of any psychological society in the last few years can fail to be impressed by the wide diversity of opinion there expressed. After fifty years of apparent progress American psychology has reached a point where two psychologists wishing to carry on a professional discussion must spend most of the time defining the terms they are using. As Hull has said, "One of the most striking things about the theory of learning and of psychological theory in general is the wide disagreement among individual psychologists" (8). The confusion is so great that already leading psychologists have predicted the impending dismemberment of psychology.

Controversy over clear-cut and well defined points of view is a necessary prelude to progress in the development of any science. It does not appear, however, that the 'wide disagreement' cited by Hull above holds the promise of an impending step forward in the history of psychology. It is more likely that the confusion is a symptom of defeat and of the present inability of the science to live up to its early promise, a failure that is implicit in Pratt's statement that "For a long time to come psychology should remain in the laboratory and the library" (13).

Some writers have sought to gloss over this chaotic situation by insisting that the disagreements concern theory only and that there is little difference over questions of fact. This evasion can be of little consolation to the vast number of parents, teachers, physicians, and other workers in the applied fields who must depend upon theory for guidance in their attempts to predict and control human behavior.

Prediction of new phenomena is necessarily based upon theory, not directly upon the results of previous observations. Knowledge of the results of a large number of discrete experiments, without theoretical interpretation, offers no way of predicting results when the experimental situation is different in the least degree. It may be anticipated that the differing conditions may lead to different behavior, but the kind and degree of difference are unpredictable without some theory of causation. Such circumstances are universal in any field of applied psychology where the teacher, the clinician, or the psychiatrist is always endeavoring to predict and control behavior in situations which are in some degree new and unique. Applied psychology is in vital need of principles of prediction that are simple, accurate, and unambiguous, that can function as principles for prediction rather than as myths for explanation, and that can be applied with precision in situations never before investigated.

It is precisely at these points that the current body of psychological theory is defective. It is complex, cumbersome and contradictory, and each of the conflicting systems is itself a welter of contradictory principles of causation and description. Catching phenomena coming and going, these systems serve nicely as explanatory concepts after the event but, because of their antagonistic character, are relatively useless for prediction. Ego-id, primacy-recency, similarity-contrast, differentiation-integration, facilitation-inhibition, learning-forgetting, self preservation-death wish are only a few examples. In the effort to explain all known facts every current system has been forced into the use of conflicting principles such as these. Each system has a large number of principles because it must account for a large number of facts; and in each system some of the principles are conflicting because the facts which they attempt to explain are conflicting. It is the thesis of this paper that this apparent conflict is an artifact. The facts are conflicting because they are derived from two mutually exclusive frames of reference.

It has become a common-place in physics that the obtained figures for the mass, velocity, and path of any object are

dependent upon the point from which they are observed. It is less widely recognized that the same principles apply to the collection of psychological data. For whatever purpose behavior is to be studied it must be observed from one of two distinct points of view. It may be studied objectively, as by an outside observer; or it may be studied phenomenologically, from the point of view of the behaving organism itself (17). The facts derived from these two points of view are nonidentical and are often completely contradictory. As seen by an outside observer, for instance, learning is a process of progressive change in the learner's response to a static situation. During this process the situation or task remains unchanged while the behavior becomes more relevant and efficient. Since the situation remains unchanged, improvement is ascribed to hypothetical changes within the learner. Thus the objective approach inevitably includes among its derived facts random molar behavior and improvement with repetition, usually by association or integration. Educational procedures based upon objective facts customarily stress frequency, drill, reward and punishment.

From the phenomenological point of view, that is to say, from the point of view of the learner, the facts are quite different. The learner remains unchanged. It is his experience of the situation or task which changes. From his point of view his behavior is always insightful, that is to say, it is always relevant to the situation as he interprets it at the moment. Improvement is concurrent with changes in the observed nature of the task, usually described as differentiation, individuation, or increase in detail. These data are in direct contradiction to the data derived from the objective approach; the observed facts of one frame of reference completely contradict the observed facts of the other. Educational procedures based upon phenomenological facts also differ from those based upon objective facts since they stress understanding of the individual child, pacing, and clear presentation of material.

Twenty years ago the point of view in American psychology was predominantly objective. An attempt was being made

by one school, the behaviorists, to construct a methodologically pure theory of learning from that point of view. Since then, largely due to the influence of the Gestalt movement, the trend has been toward an increasing acceptance of facts derived from the phenomenological point of view. Unfortunately the trend has, in most cases, manifested itself in the attempted assimilation of phenomenological facts into a basically objective theory.

An analogous situation might have arisen during the shift of astronomers from the Ptolemaic to the Copernican frame of reference. If we take our place of residence as the fixed unmoving point of observation, it follows as a necessary and undeniable fact that the sun revolves around the earth. It is a fact that can be verified by independent observation on any clear day. On the other hand, if the sun is taken as the fixed point of reference it is an equally observable fact that the earth revolves around the sun. Both facts are not only verifiable by observation but can be used in the prediction of phenomena, for example eclipses. They are not, however, co-existent facts since they are derived from two mutually exclusive points of view. It is not likely that during the long controversy over the 'truth' of the two systems any eclectic ever had the hardihood to predict an eclipse from a synthesis of Ptolemaic and Copernican facts, with the earth going around the sun while the sun went around the earth and both stood still.

Precisely such a synthesis, however, is continually attempted in psychological discussions of learning. A glance at any current text will show how *error*, *reflexes*, *stimuli*, *frequency*, and other data derived from the objective point of view are complemented with such phenomenological concepts as *trial*, *insight*, and *belonging*. The statement found in a widely used outline for students, that we learn usually by trial and error but sometimes by insight, is only an extreme example of this generous and uncritical point of view. Nor should it be supposed that the indiscriminant amalgamation of objective and phenomenological data is confined to the works of avowed eclectics. 'Effect' and 'consequences' have long done duty in both camps, referring on the same page to changes in the

structure of the organism and to changes in the way the learner experiences the task. Even Gestalt psychologists have failed to recognize the non-simultaneity of the derived data. Koffka's invocation of the 'geographic' environment (9), necessitated by his desire to assimilate data derived from an objective approach, is a case in point. By its introduction into an essentially phenomenological system it is possible for him to take cognizance of items which exist in the phenomenal field of the experimenter but not in that of the behaving organism.

Given a theory of learning based upon one of the two points of view, an assimilation of facts derived from the other can be achieved only by introducing additional and necessarily conflicting laws and concepts. This was done by Thorndike (effect-belonging, 21) and, more recently, by Maier (learning-reasoning, 11) in adding phenomenological data to objective systems. Koffka and Freud, in adding objective data to phenomenological systems, attempted to avoid the uncertainty involved in a multiplicity of independent processes but found it necessary to postulate new loci of action (geographic environment, unconscious) not open to direct observation by either the observer or the participant. Uncertainty in prediction arises, on the one hand, from the multiplicity of independent laws, and on the other, from the circumstance that the postulated law is held to be operating in a field inaccessible to observation. In either case the increased possibility of a plausible explanation for any specific act is more than counterbalanced by the decrease of certainty in prediction. Mixed systems are therefore better suited for explanation in retrospect than as principles for prediction in advance. Data derived from the two frames of reference are not co-existent and a genuinely predictive psychology will be impossible of attainment until we stop treating them as if they were. Such a psychology must be either completely objective or completely phenomenological. It is the purpose of this paper to examine the value of data derived from each type of approach for their comparative value in prediction.

II. Inadequacy of the Objective Approach For Prediction of Human Behavior

Of the two possible points of view it is probable that most psychologists would prefer to use the objective approach because of its record of success in the physical sciences. It does not appear, however, that any observational approach is adequate at the present time to furnish the required principles for prediction. Whatever their possibilities for the future, attempted objective systems have up to now shared the defects of mixed systems by leaning heavily upon hypothetical loci of action or by requiring an unwieldy number of independent causal principles.

As viewed by outside observers, the behavior of living organisms varies even when the environment remains unchanged. This variability has been explained in the past by postulating mind as an unseen determiner of behavior. It is more frequently explained at present by assigning the same determining function to hypothetical changes in the organism, usually in the nervous system. If instruments can be invented which will make organic states more accessible to observation, the latter concept may prove to be a very fruitful one; but up to the present time both concepts have functioned almost exclusively as explanatory concepts, being relatively useless for prediction. Accurate prediction is possible only when the causal entities are open to inspection.

A tempting alternative to the use of unexplored causal fields is that adopted by the early behaviorists who attempted to refrain completely from causal inferences and to restrict themselves to data which could be objectively observed. Because of the notorious variability of animal behavior under objectively identical conditions such attempts to discover purely descriptive laws have not, however, been very successful. A common and necessary assumption of the objective way of search has been that the apparent irresponsibility of living organisms to physical causation is due to the gross character of the units studied. Further analysis, it is hoped, will show the parts of the organism functioning in ways

predictable by an adequate physics. By withdrawing from the study of organismic behavior into the study of part behavior, of reflexes, or of S–R bonds, it is possible to maintain the concept of lawful causation of events and at the same time maintain the objective approach. This procedure, however, involves an indefinite multiplication of causal processes, with attendant confusion in prediction. Since it is possible to investigate the relation between an animal's behavior and any feature of a situation which can be experienced by the experimenter, present day objectivists are embarrassed by a plethora of causal factors. Buel (4) has reviewed eighty-three factors which have been found to affect the pathway chosen by a white rat approaching a point of bifurcation in a maze. He points out that the list is not exhaustive and the eighty-third factor is 'chance.' The hopelessness of using such a large number of independent principles as bases for accurate prediction is obvious.

The situation, then, is this: From the objective point of view, behavior which is not pertinent to the situation as viewed by the experimenter is random, indeterminate, fortuitous. To accept this indeterminism, however, as final would involve the abandonment of all hope for accurate prediction. Any science which hopes to predict must postulate lawfulness. Lawfulness in an objective system, however, can be maintained only by postulating additional causal agents unseen by the experimenter, as mind, past experience, instincts, or organic change. As long as these entities remain inaccessible to the experimenter they can be endowed with any necessary characteristics and are ideally suited to function as explanatory concepts. But, conversely, as long as these agents remain inaccessible the systems of which they are parts will have vital gaps in their causal fields with consequent inaccuracy in prediction. Since the unseen agents are usually invoked to explain individual variations in behavior, objective systems are apt to restrict themselves in practice to the prediction of normative behavior, concerning themselves chiefly with the establishment of norms and coefficients of correlation. This knowledge of what 'most people,' 'the average individual,' or 'the typical three-year-

old' is most likely to do in a given situation 'other things being equal' is of little value, however, to the applied worker, the clinical psychologist, or the classroom teacher who must predict and control the specific behavior of particular individuals. If the analysis of this paper is correct, the accurate prediction of such specific individual behavior, from an objective point of view, will have to wait until one of the explanatory agents, most probably the physical organism, is laid open to observation by methods and instruments not yet devised.

Pending the perfection of these devices, it appears desirable that an attempt be made to explore the possibilities of the alternative point of view, that of the behaving organism. The remainder of this paper is devoted to a discussion of a phenomenological system that has been used with some success in predicting previously unobserved behavior (**18**, **19**, **20**). The discussion is restricted roughly to the field of learning, which is the most crucial to the problem of prediction.

The reader will bear in mind that the 'facts' of such a system will necessarily conflict with those derived from the objective point of view and that the validity of any frame of reference must be judged, not by the degree to which its facts correspond to the facts derived from other approaches, but by its usefulness in prediction.

III. The Characteristics of a Phenomenological System

1. *The basic postulates.*—Assuming that the task of psychology is the prediction and control of behavior, a phenomenological system must rest upon three basic assumptions [1] and three principles.

A. All behavior is lawful. This is a necessary assumption of any system, since chance behavior would be unpredictable.

B. Behavior is completely determined by and pertinent to the phenomenological field of the behaving organism. By phenomenological field, hereafter abbreviated to p.f., is meant the universe, including himself, as experienced by the behaver at the moment.

[1] The first assumption is common to all scientific systems, the second and third are matters of direct observation but impossible of proof.

C. There is some relationship between the phenomenological fields of different individuals. This is a necessary assumption, since control is impossible if one individual is unable to affect another's field. The locus of the relationship, usually presumed to be an underlying reality, is not open to observation.

D. Greater precision of behavior (learning) is concomitant with greater differentiation of the phenomenological field. Another characteristic of p.fs. is that they are fluid and shifting; their phenomena are continually reshaped and given new meanings by the character of the total configuration. Memories, for example, are strongly affected in this way (1). Maier (11) found that the crucial act of solutions was forgotten as soon as the solution was made; and Wees and Line (23) found that school children, in the act of reading a story, distorted its details in ways that made it more meaningful and pertinent to their own experiences. Since behavior is part of the field, taking part in the field's interaction, principle *E* is in some ways a restatement of the second postulate *B*.

E. The characteristics of the parts of the phenomenological field are determined by the character of the field itself. More specifically, the direction and degree of differentiation are determined by the phenomenological needs of the behaver.[2] The reader may find, for example, that in reading this paper he has been particularly aware of the points which substantiate his own views. The fundamental need in a phenomenological system appears to be the preservation of the organization and integrity of the p.f. and especially of that part of the field which is the phenomenal self, whence our tendency to remain unaware of, or to reject with emotion, data inconsistent with our own beliefs.[3]

[2] Both *D* and *E* invalidate introspection by the learner as a means of reconstructing his own field. Much of the field is too vague and undifferentiated (*D*) to be verbalized; and the need to observe and report may considerably alter the character of the field (*E*) and the nature of the problem.

[3] This recognition that the self we are trying to preserve is the phenomenal self, that is to say, is our own picture of ourselves, explains the need which various schools have described as drives for self-esteem, self-respect, security, status, superiority, power, or complacency. When self-preservation is thus referred to the phenomenal self, it is adequate for the explanation of suicide and martyrdom. These two forms of behavior have always been a source of difficulty from a systematic point of view. Objective systems have been forced to ignore them, along with other un-normal

F. Differentiation takes time. It follows from this principle that the way to accelerate learning is to arrange the situation so that the required differentiations are either more obvious or are unnecessary. For instance, in a black Warden multiple-U maze of the LRRRLLRLLR pattern which had been learned by a group of white rats in a median of 29 trials, the application of white paint to the critical 2, 5, 7, 8, and 10 sections, where changes in procedure were necessary, enabled an experimental group to learn the maze in a median of 12 trials (19). When the differentiation of individual sections from one another was made completely unnecessary by painting the blind alleys white and the correct pathway black, or vice-versa, the median number of trials required for learning was lowered to 7 (18).

2. *The problem of prediction.*—By postulate *B* the determining locus of action is the behaver's p.f. This is not open to direct observation by any outside observer. The process of prediction therefore involves two steps: (1) the securing of an understanding of the subject's field by inference or reconstruction, (2) the projection of the future field.

The first operation is of the common "Now why did he do that?" or "Under what circumstances would I have done that?" character. Much of the topological work of Lewin is of this type and essentially the same procedure was used by Shepard (15) when from the behavior of his rats he inferred the existence of floor cues which he himself was unable to experience. The teacher who hears his pupil report that 3×0 is 3 and infers that his reasoning is "Zero is nothing so it does nothing to the three" has taken this step. The operation acquires its validity in this system by the postulate (*B*) that behavior is completely determined by the p.f., whence it follows that variations in behavior are always indicative of concurrent variations in the field. The complete operation of prediction imposes two important conditions. To reconstruct an individual's field from his behavior it is necessary to have some idea what fields are like, and to

behavior, and mixed systems can include them only by postulation of independent motives conflicting with self-preservation, such as Menninger's death wish (12).

project the future field it is necessary to understand how fields change.

3. *The nature of the field.*—The p.f. is simply the world of naive, immediate experience in which each individual lives, the everyday situation of self and surroundings which the unsophisticated person takes to be real. Studies on the nature of this field indicate that all parts of the field are not equally distinct. The field consists of figure and ground, or focus and margin; there are not two definite, static levels, but one level may shade into the other so that the figure may be large and relatively indistinct or small and highly differentiated. Experience in any sense field can be figure. Pain, fatigue, or the disturbed organic states involved in emotion may emerge so sharply as the focus of the field that all the rest of the field fading into the homogeneity of ground, that the individual will lose touch with his surroundings and become unconscious. Since by postulate *B* behavior is completely determined by the p.f., a highly detailed and differentiated field will include definite and precise behavior, while, as anyone who has tried to find a snap switch in a strange room in the dark will agree, behavior in a vague and undifferentiated field is vague and confused. This leads to principle *D*.

4. *How fields change.*—Principle *D* of our system identified differentiation with learning; principle *E* made the determinants of differentiation somewhat explicit. Differentiation may be defined as knowing a difference, the basic act of knowledge. It is the manifestation of the continuous process by which the integrity and organization of the field are maintained. "When an individual, rat or human, is confronted with a task . . . the general procedure is determined by his initial perception of the nature of the problem; it is a gross response to a relatively undifferentiated situation. Should the first procedure, the response to the gross situation, prove inadequate the task is differentiated perceptually into segments each of which is solved by simple procedures" (19).

Although he is aware that his own field may be affected (*E*) by his desire to maintain the predictive advantages of

having only one process in the system, the writer feels that differentiation may be safely assumed to be the only process of change in the p.f. The emergence of a new entity or character into figure implies the lapse of other characters into ground. Both are necessary for the existence of a difference and are not two independent processes but complementary aspects of the same process, which might be called 'change.' Since, however, it is the newly emerged figure, the focus of the behaver's field, which is the most directly potent in determining behavior, it seems more practical to emphasize the more effective aspect of the process and call it 'differentiation' or 'individuation' rather than the non-valuative 'change.'

The basic assumptions and principles are now complete. There remains to be discussed principle *F*, which is subsidiary and based on experimental evidence (**3, 16**).

5. *Use of the system in prediction.*—Several examples of the use of this system for the prediction of previously unreported animal behavior have been published (**2, 18, 19, 20**), the last of which may serve as an example. The purpose of the study was to test the comparative usefulness of phenomenological and objective principles in the prediction of maze behavior. Since DeCamp's experiment (**5**), reported in 1920, it had been almost axiomatic that rats tend to adopt the shorter of two alternative paths to food. Now suppose that the structure of a rat's field is such that a path which is objectively the longer of two alternative paths to food appears to the rat to be the shorter. If, as we have postulated (*B*), the animal's behavior is completely determined by his p.f. he will choose this phenomenologically shorter path in spite of the fact that it appears to the experimenter to be the longer. According to objective principles, on the other hand, the effective determinant would be the physical lengths of the two paths 'other things being equal,' irrespective of how the situation was experienced by the rat.

Such a situation was arranged in a ten section Warden U LLLLLLLLLL maze, in which an alternative food-box was attached to the right hand alley of the third section. Thus

it was possible for the rats to take either a short three section LLR path or a long ten section LLLLLLLLLL path to food. If the behavior were governed by the objective situation it would be predicted that they would choose the shorter LLR path. However, previous experiments from a phenomenological point of view had led to the inference that "maze learning is a process of increasing differentiation in the total situation, during which the maze is first perceived as a general path to the food-box and is then differentiated into submazes. . . ." (19). It was therefore predicted that in this maze the animals would at the end of the first trial experience the path to either food-box as a general leftward path. As a consequence they would take the longer LLLLLLLLLL path on later trials. The experiment was made by running a group of rats in this maze once a day for 100 days. During the first 75 days the animals verified the prediction by taking the longer path on 74 per cent of the runs. During the last 10 days the longer path was taken on 89 per cent of the runs. This behavior conformed to the inferred p.fs. of the rats, thus demonstrating the usefulness of the postulates used in its prediction.

In the same way, it was predicted that animals in a similar maze where the alternative food-box was on the right hand alley of the second section, which is directly in front of the maze entrance, would experience this LR path as something like 'ahead' or 'around the corner.' This would not lead them to take the long leftward path. This prediction, also, was verified. The rats in this maze took the shorter path on 64 per cent of the runs during the first 75 days, and on 66 per cent of the runs during the last ten days.

6. *The conditioned response.*—The only real and valid test of any system in science is its effectiveness in predicting previously undiscovered phenomena, as in the case just cited. Ability to explain phenomena already known is, of course, no criterion of usefulness in prediction. It is true, nevertheless, that inability to explain known phenomena is presumptive evidence of inadequacy in prediction as well. For this reason it is interesting to test the adequacy of the proposed

system to explain some of the phenomena of the conditioned response, upon which objective systems commonly base their description of learning. A phenomenological explanation would be something like the following, the capital letters referring to the principles involved:

(1) Conditioning.

a. Objective description: Given an animal with a need (*e.g.*, for food), and a means of affecting that need (*e.g.*, food). Present a signal (*e.g.*, a tone) of a kind that in sufficient volume and under favorable circumstances is capable of eliciting a response from the animal, then, in fairly close temporal and spatial contiguity, present the means of affecting the need (the 'unconditioned stimulus'). After one or more presentations the signal will elicit the same response as the unconditioned stimulus or a response similar to it.

b. Explanation: Given time (*F*) and a need to be satisfied (*E*), the signal and the unconditioned stimulus will be differentiated as a unit from a relatively homogeneous field. Since the degree of differentiation required is determined by the precision of response required (*D*) signals for diffuse bodily and postural activity may be effective at a very low level of awareness. The closer the signal and unconditioned stimulus in the experimenter's time and space, the more apt they are to be differentiated as a unit from the rest of the learner's field (*C*). It would not be expected (*B*) that the response to the signal-stimulus unit would be exactly the same as to the stimulus alone. In a class demonstration with a human subject, using a strong buzzer tone one second before a strong shock to the right fore-finger from a curved finger rest, the unconditioned response was an extension of the finger. The conditioned response, however, was flexion together with a lifting of the finger, the whole action taking it off the grill. Phenomenologically the subject was bracing himself for the expected shock.

(2) Irradiation.

a. Objective description: If the signal is altered within a varying range the established response will still be made.

b. Explanation: If the signal is incompletely differentiated it may be confused with other signals. If it is experienced as a vague feeling of discomfort or expectancy a large number of signals in different sense fields might elicit the response. If it is more clearly individuated there will be less opportunity for confusion with other signals.

On the basis of this inference that 'irradiation' and 'generalization' are incomplete differentiation, it would be predicted that continued presentations of the signal-stimulus unit will, by giving more opportunity for precise differentiation (*F*) of the signal, lessen the number of signals with which it might be confused and diminish the probability of 'irradiation' responses. This prediction corresponds to the results reported by Razran (14, see pages 7 and 8), although it does not agree with his interpretation.

(3) Differentiation.

a. Objective description: Both signal A and signal A′ elicit the response. If signal A is given with the unconditioned stimulus and signal A′ without it, the latter signal will cease to elicit the response.

b. Explanation: Presentation of signal A′ without food makes it necessary (*E*) and possible (*F*) for the subject to differentiate it from signal A.

(4) Unconditioning.

a. Objective description: If the signal is given without the unconditioned stimulus or some time after it, it will, after a varying number of presentations, cease to elicit the unit response.

b. Explanation: Separate presentation provides the subject with an opportunity for differentiating them from one another (*C*) (*F*). The signal from a signal-food unit should have no effect even the first time it is given after food if the need for food has been completely satisfied (*E*). The signal from signal-punishment units, however, would be effective for several presentations when given after the punishment, since the need to escape punishment is not satiable. Repeated presentations, however, will give an opportunity for differ-

entiation and the signal will eventually emerge as a cue that punishment has ceased and will then evoke a different response.

(5) Pseudo-conditioning.

a. Objective description: "In some cases a response is elicited by a formerly inadequate stimulus (signal) which has been preceded by an unconditioned stimulus" (7).

b. Explanation: In cases where the unconditioned stimulus is one, like punishment, that leads to a continuing state of need and tension, the animal will differentiate out and respond to features of the field previously ignored. The signal is effective because it is unfamiliar, that is to say, it is incompletely differentiated from the shock situation. "The animals gave the impression of responding *as if* a shock were expected and *as if* they knew no appropriate response to make" (7, p. 372). In cases where the unconditioned stimulus satisfies the subject's need pseudo-conditioning will not occur.

7. *Other concepts of learning.*—One of the greatest advantages of the phenomenological method is that it is able to bring the experimenter within the scope of its system and to apply its laws to the observer himself. When examined in this manner, most of the conventional concepts of learning prove to be products of the relationship between the p.fs. of the observer and the learner. For example: If the observer's field is more highly differentiated than the learner's, the latter's less precise behavior (*D*), since it does not conform to the situation as experienced by the observer, is said to be *error*. Phenomenologically errors are recognized only in retrospect, that is to say, when an individual compares his past behavior with his present more highly differentiated field.

If the learner's field closely approximates the field of the observer, so that the learner does what the observer would do, the behavior is said to be *correct* or *insightful*.

If the observer's field is less differentiated than the learner's there are two possibilities: (1) The learner's more precise and efficient behavior may lead the observer to discover features of the situation of which he had previously been unaware; in which case a third party might infer that

the observer had learned by *imitation*. (2) The learner's mysteriously precise behavior in what to the observer is a relatively undifferentiated field may lead to the assumption that the behavior is determined by *instinct*. The farther removed an animal is from the human in sensory and behavioral possibilities, and the more difficult the reconstruction of its field, the greater the chances that instinct will be invoked as an explanation for its behavior.

8. *Association or differentiation?*—If an observer in a highly differentiated field, which he naively takes to be real, watches the behavior of a learner he is certain to interpret the process as one in which the highly individuated items in his own field are *combined*, *organized*, *associated*, or *integrated* by the learner. Because he believes his own field to be real, he postulates that the learner's field is made up of the same items, but in an inferior state of organization. Objective theories of learning, therefore, begin with an unorganized field and attempt to show how it becomes organized.

Cross section studies of the learner's p.f. at different stages of learning, however, show that the field is always organized. The change lies in increasing detail rather than organization. Entities are thus 'associated' when they are incompletely differentiated, having emerged together out of a common ground to satisfy a need. They are experienced as contiguous when they are incompletely differentiated in space or time and as like or opposite when they emerge in some mutual relationship. The so-called laws of association: *contiguity*, *similarity*, and *contrast* thus become descriptive of incomplete differentiation in the phenomenological field.

9. *Frequency*.—Frequency is the basic but unreliable causal factor of most objective systems. From the phenomenological point of view it may afford an opportunity for learning (*F*) but cannot cause or guarantee it. What is learned depends upon the phenomenological needs of the learner (*E*). This accounts for the success of Dunlap's method of breaking habits by practicing them (**6**, **22**).

10. *Relationship to the physical sciences*.—One of the most interesting implications of the phenomenological approach is

the status it assigns to the physical sciences. Physicists secure regularity and lawfulness by restricting themselves to a common and rigorously limited phenomenological field. They share this common p.f. (sometimes called the physical universe) by the process of taking all measurements from a standard position in front of their instruments and confining their observations to the pointer-readings thus derived. This process gives to physical scientists the unique advantage of a common field; but it is a field where all the characteristics of phenomena are eliminated except those reducible to length; where light, color, temperature, taste, and odor exist, if at all, only as readings on a scale; and where song, oratory, poetry, and propaganda are alternating areas of condensation and rarefaction. As long as the physicists can remain in this common field their behavior is identical. They make the same calculations and predictions because they are living in identical fields.

This use of mediate observation in the physical sciences has led to spectacular agreement in some fields. It is, however, essentially a search for agreement by elimination of phenomena and is therefore limited in its possibilities. Since the phenomena that must be excluded are essentially those necessary for the prediction and control of human behavior, it is becoming apparent that the methods of the physical sciences cannot be taken over bodily by the psychologists.

11. *Advantages of the phenomenological approach.*—Philosophically a phenomenological system has a number of advantages, several of which have been enumerated or implied. From the point of view of practical use for the prediction and control of behavior, however, four are outstanding:

(1) A phenomenological system is anthropomorphic. Its data are stated in terms of immediate experience and require no translation to make them meaningful.

(2) It is concerned with the prediction and control of individual behavior, a field closed to objective systems because of their necessary assumption of variability in individual behavior. For this reason psychiatrists, applied psycholo-

gists, and teachers when dealing with individuals commonly adopt a phenomenological view. This accounts for the great use by these groups of psychoanalysis, Gestalt, and private non-academic systems which have large phenomenological components. The use of a general field can result in the prediction of general, normative behavior only.

(3) As compared with the objective approach, the phenomenological approach is more inclusive. Individual behavior cannot be predicted from normative behavior. On the other hand, accuracy in predicting individual behavior makes possible the prediction of normative behavior as well.

(4) The particular system outlined in this paper has the predictive advantage of postulating only one process, which is descriptive rather than causal or explanatory.

12. *Relation to other systems.*—It is impossible to say whether this system is the only possible phenomenological system. The first three principles are certainly basic and the next three appear to follow. Gestalt psychologists and their pupils have made the major recent contributions in this field and have failed to achieve a purely phenomenological system only because of their failure to appreciate its necessity. As soon as non-phenomenological data are eliminated from Gestalt psychology, it becomes the system that has been described.

Psychoanalysis, the other semi-phenomenological system, differs basically in its interpretation of the nature of the field. Freud recognized the figure-ground character of the field and the importance of the ground in behavior but, lacking experimental evidence, distorted the figure-ground relations into an antagonistic dichotomy in which the ground (unconscious) had all the characteristics of figure (conscious) including a self (id). Since these characteristics are not experienced by the individual the system ceased to be purely phenomenological. The methods of analysis are directed toward securing an understanding of the patient's field and have resulted in the accumulation of important data, especially in the area of motivation. Since the conscious-unconscious dichotomy is the cornerstone of the system it is impossible to see, however,

how psychoanalysis can evolve into the purely phenomenological system that is needed.

IV. Summary

The current confusion in psychology is largely due to the uncritical combination of data from two different frames of reference. Facts derived from a phenomenological point of view are non-identical with and often completely contradictory to facts derived from an objective point of view. Systems attempting to combine facts derived from both frames of reference are forced into a multiplicity of conflicting laws and concepts or into the postulation of loci of action inaccessible to observation. In either case accurate prediction of behavior is impossible. Objective systems, although philosophically more satisfactory than mixed systems, have been forced in practice to confine themselves to the prediction of normative behavior. For this reason psychiatrists, applied psychologists, and teachers, when dealing with individuals commonly adopt a phenomenological view. This accounts for the great use by these groups of psychoanalysis, Gestalt, and private non-academic systems having large phenomenological components. The best immediate prospects for a psychology able accurately to predict individual behavior lie in the development of Gestalt psychology along purely phenomenological lines.

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DECISION-TIME IN RELATION TO THE DIFFERENTIATION OF THE PHENOMENAL FIELD[1]

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I. Introduction

This study reports the theoretical aspects of an investigation which may be taken as a clear example of the hypothetico-deductive method. From concepts developed in the psychology of motivation, logical derivations were made in regard to certain phenomena in the field of perception. Accordingly, experiments were designed which actually bore out these derivations (7) and, in addition, a number of experimental data which had previously appeared rather chaotic became comprehensible in the light of these derivations.

A delay in reaction has been taken by psychologists in widely different fields of study to be an indication of conflict within the individual. For Ach (1) an increased reaction-time signifies that a 'tendency' of the will and a 'tendency' of association have come into conflict; and although Lewin (22) disagrees with many of Ach's theoretical interpretations, he employs the same criterion of conflict in his own experimental studies. This criterion, moreover, is utilized in Jung's 'complex indicator' (17), where a slow response is considered to indicate that the normal association has come into conflict with an emotionally toned complex. Animal psychologists, too, have employed the criterion implicitly, at least, when measuring the strength of motivation. In obstruction-box experiments, for example, they measure the strength of a drive by opposing it to a standard obstruction, and they often

[1] The material here presented is included in the author's thesis which was submitted to the Department of Psychology, Harvard University, as partial fulfilment of the requirements for the degree of Doctor of Philosophy. The author here acknowledges his indebtedness to Professors Kurt Lewin and E. G. Boring for their helpful assistance throughout the investigation and to the author's wife, Barbara Cartwright, who drew the figures presented here and in the thesis.

consider the frequency with which the obstruction is crossed within a given interval of time as a measure of the strength of the drive. That this measure depends logically upon the assumption of a lengthening of decision-time by the opposition of conflicting forces has been demonstrated recently by Lewin (**23**, pp. 71–82).

With a somewhat different purpose in view, many measurements of judgment-time have been taken in psychophysical experiments. In these studies it has been assumed quite generally that a lengthened judgment-time indicates uncertainty of judgment and low reliability of response. With the possible exception of Carter (**5**), however, no one has proposed an explicit theory in which variations in judgment-time are explained by the same constructs employed to account for variations in other forms of reaction-time. The two sets of data have been considered to be quite unrelated.

The purpose of the following discussion is to present a general theory of decision which will allow systematic ordering of these many apparently diverse data. The theory is stated in concepts proposed by Lewin and his students because these concepts (1) represent a beginning toward the mathematical treatment of complex data, (2) are suited to an analysis of the total situation existent at a given time, *i.e.*, they permit a field theory, (3) relate immediately to a large realm of facts and theory in the wider field of motivation,[2] and (4) avoid speculation about postulated 'entities.' The term 'theory' in this study, therefore, is not intended to mean a linking of descriptive data to other entities, but rather the mathematization of these data.

II. A Theory of Decision-Time

In order to develop an adequate theory of decision it is necessary to understand clearly those basic functional relationships which are involved in any decision. This end can best be served by reference to the topological representation in Fig. 1. In the hypothetical case illustrated here, the person is about to make a decision in which he is confronted

[2] See particularly Lewin (**23**).

with only two alternatives. Topologically, then, region *P* (the person) is located within region *D* (the activity of deciding) which, in turn, is surrounded jointly by the regions *A* and *B* (the two alternatives).[3] If the person is to leave the region of decision, he must enter the region corresponding to the performance of one of the alternatives. Which alternative he actually selects, *i.e.*, which region *P* enters, depends upon the distribution of forces resulting from the valences in each of the regions, since locomotion[4] is co-ordinated to the resultant of forces.

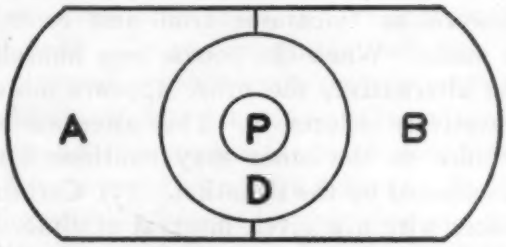


FIG. 1. A topological representation of a decision involving two alternatives. Region *P* represents the person, region *D* represents the activity of deciding, and regions *A* and *B* stand for the two alternatives.

Let us assume that the valence of region *A* is positive and greater than the valence of region *B*. The resultant of forces acting upon *P*, therefore, will be directed toward *A*, and *P* will move toward *A* with a velocity that depends upon the magnitude of the resultant. If we assume, on the other hand, that the valence of region *A* is *equal* to the valence of region *B*, the resultant of forces will equal zero and no motion can occur. In other words, as the conflicting forces become more equal, locomotion is increasingly retarded until the forces become balanced and it stops completely. In this extreme case a decision cannot be reached until some change in the situation brings about an imbalance of the forces. Since time will be consumed before this imbalance occurs,

[3] The same method of representation is employed by Korsch-Escalona (20) and by French (13).

[4] In the present paper 'locomotion' is defined as a 'change of position in topological space.'

the decision-time will necessarily be lengthened when there is an initial equivalence of the opposing forces.[5]

This simple statement is, of course, insufficient for predicting *how much* longer decision-time will be. To make the theory more specific it is necessary to enumerate the factors which bring about the imbalance of forces. A few may be suggested here. (1) When equal and opposing forces operate upon a person, he may begin a process of comparison and consideration. Viewing the alternatives in a more detailed way or in a different light may change the valences so that the balance is destroyed. (2) In many instances, probably in those known as 'vicarious trial and error,' tentative choices are made. When the person sees himself as having selected one alternative, the other appears more appealing and he tentatively selects it. This alternation from one tentative choice to the other may continue until a 'final decision' is enforced by the situation. (3) Certain situations compel choices within a given interval of time. This compulsion forces tentative choices to become final and it may also operate to reduce the amount of imbalance required for locomotion. (4) The presence of opposing forces probably places the person under tension which, in turn, creates a new force away from the region of decision. This new force would affect the decision in the same way as the force in (3). Undoubtedly many other factors may produce the imbalance. The four suggested here serve, however, to indicate areas for future investigation. Until this investigation is completed the less precise statement that decision-time increases with increasing conflict will, nevertheless, lead to certain important predictions.

The facts of judgment-time can be related to the facts of decision-time by considering the rôle of the conflict of forces in both types of situations. The theory outlined in the preceding paragraphs is intended to apply wherever opposing forces operate upon the person. Many phenomena, which at first sight appear unrelated to what we usually call 'decisions,' may be seen, upon further investigation, to involve a conflict

[5] For a fuller discussion of this type of derivation see Lewin (23, p. 78 ff.).

of forces. It is here proposed that, when a person is uncertain of his judgment, conflicting forces act upon him and that judgment-time is, therefore, lengthened in the manner described above.

A more detailed analysis of how forces operate in judgments will make the proposal clearer. When a person is asked to judge whether one line is longer or shorter than another, he has to choose between 'saying shorter' and 'saying longer.' In other words, he is placed in a decision situation like that in Fig. 1. The important fact about judging, however, is that the forces which determine locomotion are themselves determined by the organization of the phenomenal field. To put it simply, whether or not the person says 'longer' depends upon the relationship which he perceives. While it is a truism to state that behavior is guided by perceptual processes, the topological representation of this fact has proven to be most difficult. In spite of this difficulty, the relation between perception and action is so striking in the act of judging that any theory which claims to be adequate is forced to deal with it. As a beginning toward a theory a distinction between the field of action and the phenomenal field will serve to specify these two factors so that their relationships to one another may be better studied.

The field of action is used to represent the immediate determinants of action, and most of the topological analyses up to the present time have been concerned with this field. Fig. 1, for example, is a representation of such a field. The phenomenal field, on the other hand, is intended to represent the facts of phenomenal experience.[6] In this field, for example, visual patterns or experienced relations may be represented. Or, more specifically, if a number of objects are seen as 'belonging together,' they will be represented as having location in a common region in the phenomenal field. A special case of this general procedure will be employed in the following analysis of judgment-time where lines that are seen as 'longer' will be represented as belonging to a region

[6] See Brown and Voth (3) and Orbison (25) for an example of how the phenomenal field may be fruitfully employed.

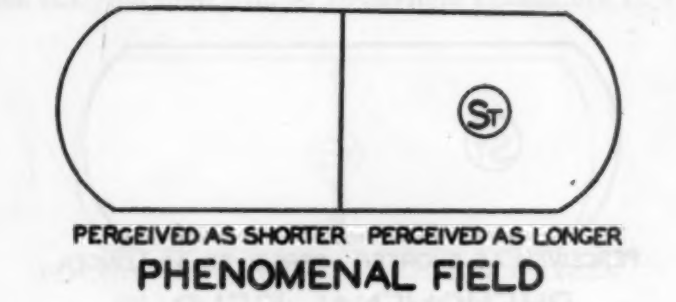
'perceived as longer.' Whether a further distinction within the phenomenal field should be made between a field of perception and a field of concepts may be left open at the present time. Should such a distinction be made, the term 'phenomenal field' as used in the following discussion would probably be changed to 'field of concepts.'

Two further differences between the field of action and the phenomenal field deserve attention here. In the first place, neighboringness of regions must be defined differently in the two fields. The field of action deals primarily with locomotion, and the structure of the field is defined by the paths of locomotion which are possible. Neighboring regions are those which permit locomotion from one to the other without the crossing of an intervening region. Neighboringness in the phenomenal field, on the other hand, is determined by the properties of the phenomenal world. Two items of phenomenal experience which are perceived as spatially adjacent, for instance, may be represented as neighboring regions in the phenomenal field. Two such items may be neighboring in the phenomenal field but not neighboring in the field of action. This discrepancy between the two fields provides the difficulty in the detour problem. In the following application of the phenomenal field neighboringness is coördinated to perceived similarity.

The second difference between the two fields lies in the definition of force. In the field of action forces are ordinarily coördinated to locomotion. Forces in the phenomenal field, on the other hand, are usually related to restructurization (change in pattern) of that field. This distinction must not be considered an absolute one, however, since forces in the field of action are sometimes coördinated to restructuring and forces in the phenomenal field may lead to perceived movements (3).

To return now to our detailed analysis of the act of judging let us consider Fig. 2. The situation represented in this figure is an extremely simple one in which the variable stimulus is clearly longer than the standard. Since the subject is asked to make a judgment about the nature of the

stimulus, the basic goal of a coöperative subject is 'to be right.' Now, in the phenomenal field, the stimulus-region falls well within the region corresponding to 'perceived as longer.' In the field of action, therefore, the region, "saying



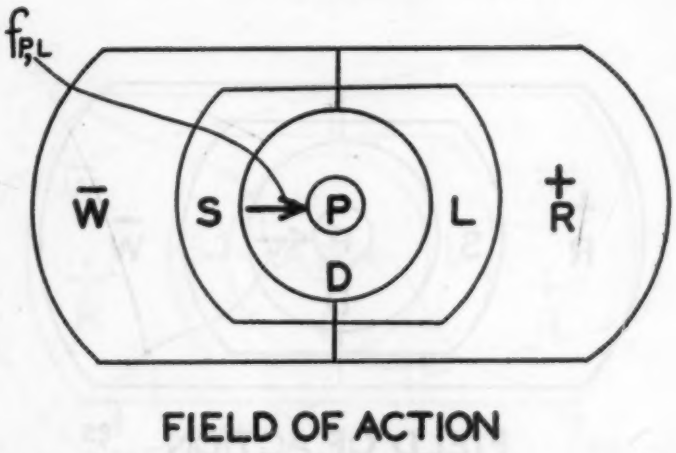


FIG. 2. The phenomenal field and the field of action when the variable stimulus is clearly longer than the standard. *P* stands for the person, *D* for the activity of deciding, *S* for "saying 'shorter,'" *L* for "saying 'longer,'" *W* for 'being wrong,' and *R* for 'being right.' The force acting upon *P* has the direction toward *L* because *L* is a part of *R* and *R* possesses a positive valence.

'longer,'" *L*, is contained within the region, 'being right,' *R*, since "saying 'longer' " and 'being right' are viewed under these conditions as equivalent. The region 'being right' possesses a positive valence, and since the regions *R* and *L* overlap, the force correlated to that valence has, at the same time, the direction toward the region $L(f_{P,L})$. A similar

analysis can be made of the region W which possesses a negative valence and contains the region S. The force coördinated to the valence will, in this case, be in the direction 'away from S' ($f_{P,-S}$). Since in this study the forces $f_{P,L}$ and $f_{P,-S}$ are always equivalent in direction, only the forces

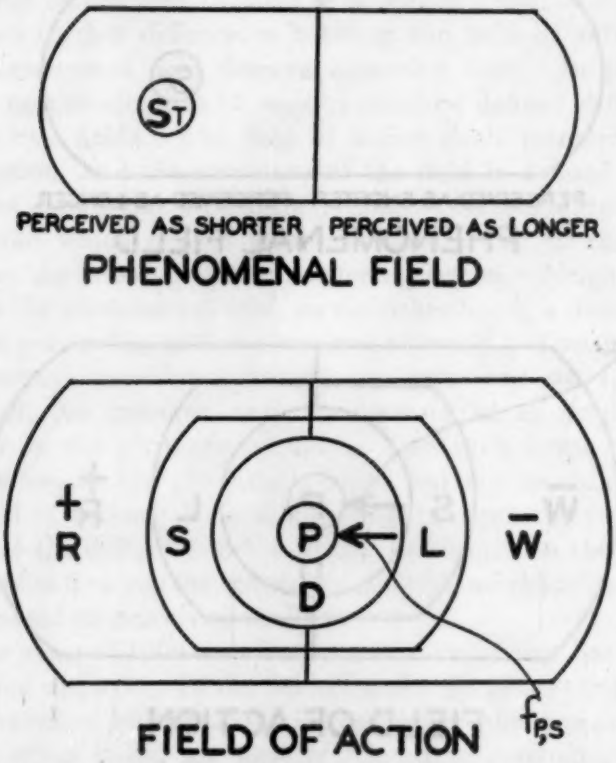


FIG. 3. The phenomenal field and the field of action when the variable stimulus is clearly shorter than the standard. The regions have the same meaning as in Fig. 2. Here, however, the arrangement of the regions and the force are different.

coördinated to the positive valence will be represented here. The force acting upon P in Fig. 2 is clearly in the direction of the region L, and the subject will, therefore, respond 'longer' without hesitation.

In Fig. 3 are illustrated the phenomenal field and the field of action when the variable stimulus is perceived as shorter than the standard. All of the forces are the opposite

of those in Fig. 2 since the region *R* ('being right') now includes the region *S* ("saying 'shorter' ") and the region *W* ('being wrong') includes the region *L* ("saying 'longer' "). The force on *P* is now in the direction of the region *S*. The subject will, therefore, respond 'shorter' without hesitation.

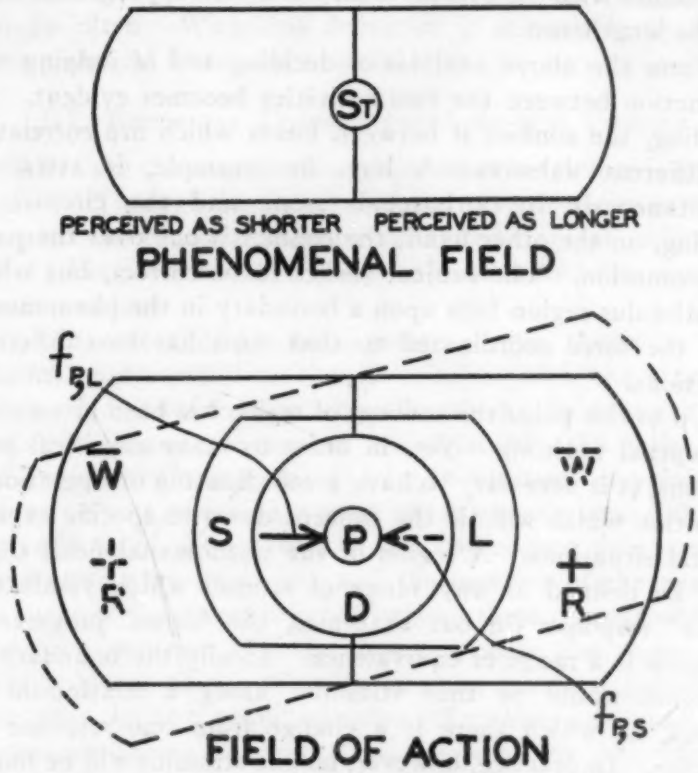


FIG. 4. A topological representation of a judgment when the stimulus is perceived as falling on the border between categories. Here two overlapping situations are operating simultaneously. The arrangement of regions and the distribution of forces in Figs. 2 and 3 operate at the same time. The person is placed in a conflict of two opposing forces. No locomotion can occur so long as the situation remains unchanged.

When the stimulus-region falls upon the boundary between the regions of 'perceived as longer' and 'perceived as shorter,' the forces are distributed as in Fig. 4. Now, since it is equally probable that either judgment will be correct, the region "saying 'longer,'" *L*, is contained in two overlapping

regions: 'being right,' R, and 'being wrong,' W. Similarly, the region S is contained in the two regions R and W. In other words, the situations illustrated in Figs. 2 and 3 are both functioning simultaneously.[7] As a result, the forces acting upon the person have opposite directions and, in accordance with the general theory of decision, judgment-time will be lengthened.

From the above analysis of deciding and of judging one distinction between the two activities becomes evident. In deciding, the conflict is between forces which are correlated to different valences. A boy, for example, is attracted simultaneously by a baseball game and the circus. In judging, on the other hand, the conflict is one over the path of locomotion. The subject wishes to be correct, but when the stimulus-region falls upon a boundary in the phenomenal field the force coördinated to that wish has two different directions.

Up to this point the concept of region has been given only conceptual meaning. Yet, in order to make empirical predictions, it is necessary to have a coördinating or operational definition which will tie the concept down to specific experimental situations. A region of the phenomenal field, then, may be defined as any range of stimuli which yields the 'same' response (in our examples, the 'same' judgment). A region is a range of equivalence. Ideally the boundary of a region would be that stimulus, along a continuum of stimuli, at which there is a change from one response to another. In practice, however, no one stimulus will be found

[7] Korsch-Escalona (20), in her study of the level-of-aspiration in manic-depressive psychoses, develops a theory of decision-time which is strictly parallel to the one presented here. The region corresponding to the performance of any given task is included in a region corresponding to 'being successful' and is also included in one corresponding to 'being unsuccessful.' The experienced probability of success and failure determines the relative potency of the forces in the direction of each of the regions. The experiment of Johnson (16) on the relation of confidence to judgment-time may be interpreted from this same point of view. A strong feeling of confidence that the variable line is longer is equivalent to a high potency of the situation in which L is included in R (Fig. 2). A decrease of confidence means that the relative potency of this situation, or the probability of this inclusion, is decreased. Johnson's measurements of confidence, then, may be taken as an operational definition of relative potency.

invariably to serve as a boundary, thus defined, since fluctuations in the location of this boundary are always present. If it is assumed, though, that the boundary will fluctuate symmetrically around a central point, a statistical definition of the boundary of a region may be conveniently derived: it is that stimulus which leads to two responses (judgments) equally often. With this definition it is possible to apply the conceptual system to empirical data.

III. Empirical Consequences of the Theory

What are the empirical consequences that derive from this theory? Through its application can systematic order be given to the many apparently unrelated data of judgment-time? Five considerations may be enumerated in answering these questions.

1. The most direct consequence of the theory is that maximal judgment-time should coincide with the stimulus which yields two responses equally often. In order to test this consequence, the author (7) obtained 4188 judgments and judgment-times from subjects in fourteen experimental groups. From these data were made seventy-two different comparisons of the location of the 50-per-cent point on the curve of relative frequency and the location of the peak of the curve of judgment-time. Forty of these comparisons are in strict agreement with the theory, that is, the 50-per-cent point is closer to the stimulus yielding maximal judgment-time than to any other stimulus. In thirty-one cases the 50-per-cent point is closest to the stimulus immediately adjacent to the one yielding maximal judgment-time. The curves of judgment-time in most of these latter cases, however, were such that a very slight change in judgment-time for one stimulus would bring about strict verification of the theory. In only one comparison was the 50-per-cent point removed as much as two steps of the stimulus-scale from the stimulus yielding maximal time. It is highly probable that, if the number of judgments upon which each comparison is based were increased, many more of these comparisons would show strict corroboration of the theory. Remarkably strong sup-

port for the theory, moreover, is obtained from an experiment by Kellogg (18) in which 3360 judgments were recorded. A special analysis of these results shows that the relation between the relative frequency of judgments and judgment-time is precisely that predicted by the theory.[8]

The demonstration that judgment-time is maximal for stimuli which yield a relative frequency of 50 per cent reduces the apparent arbitrariness in experiments with three categories of using the distance between the 50-per-cent points as a measure of the limen. While it would still be incorrect to consider limens, thus defined, as fixed or real entities (since the 50-per-cent point is simply the most *frequent* location of the boundary), it is now clear according to the theory that the 50-per-cent point has special functional significance: it is the most frequent *locus* of the conflict between alternative judgments.

Additional information about the nature of judging may be obtained from a study of the curve of judgment-time. In the author's experiments cited above, it was found that all stimuli yielding relative frequencies of 00 per cent do not produce the same judgment-times. Rather, as the stimuli approach that stimulus which yields a relative frequency of 50 per cent, the judgment-time increases even though the same judgment is always given. In other words, a conflict of forces begins to appear, and thus to retard action, even when the resultant of forces is always in the same direction. The curve of judgment-time, therefore, is in certain respects a more sensitive measure of conflict than the curve of relative frequency.

2. The theory is intended to apply not only to judgments in the traditional psychophysical experiments but to any judgments which depend upon the differentiation of the phenomenal field. That is to say, the same relationships between frequency of response and judgment-time are expected wherever ranges of equivalence are involved. In the course of testing the relationships predicted by the theory, the author (7) studied the ranges of equivalence produced by

[8] See Cartwright (7); especially Fig. 26.

(*a*) the organization of the visual field (differential sensitivity), (*b*) various conditions of learning, (*c*) the subject's normal use of words in sentences, and (*d*) the operation of political attitudes. Under all of these conditions not only was it possible to discover ranges of equivalence and to apply the concept of regions within the phenomenal field, but the theoretical predictions were also satisfactorily verified.

3. The theory is found to apply, moreover, to data collected by other investigators who had quite different problems in mind. What, for example, will be the relationship between the number of categories employed and the average judgment-time for all judgments? Kellogg (18) found the average time to be longer for three categories than for two. In the light of the present theory the reason is clear. When only two categories of response are used, there are two regions within the phenomenal field and only *one* boundary between them. As a result, the curve of judgment-time possesses only one peak and the *average* time for the judgments is raised only slightly above the lower values. When three categories are employed, however, there are three regions and *two* boundaries. Now, the curve of judgment-time possesses two peaks, and the *average* time for the judgments is raised considerably above the lower values. Thus, Kellogg's finding that the average time is longer for three than for two categories of response provides further support for the theory.

4. In the past the related problem of whether or not to include the equal-category in psychophysical experiments has been the cause for recording many measurements of judgment-time. It was thought that longer judgment-times for judgments of equality than for judgments of difference would demonstrate that doubt and unreliability were associated with the central category and would thus provide justification for its elimination. Several investigators (24, 14, 10, 18) did, in fact, find longer times for judgments of equality, but other investigators (12, 4) found no significent difference between the judgment-times of the various categories. These conflicting findings were finally reconciled by Fernberger, Glass, Hoffman, and Willig (11) when they demonstrated that the

different findings resulted from the operation of different attitudes toward the category of equality. When the experimental conditions led the subject to feel that he should not give a judgment of equality unless absolutely necessary, the judgment-time for that category increased. When, on the other hand, the subject accepted the category of equality upon a par with the categories of difference, the judgment-times were the same for all of the categories.

This reconciliation of the apparently divergent findings permits a general interpretation of all the experiments in terms of the present theory. Fernberger (9) has shown that the attitude which a subject directs toward the central category determines the size of the range of equal-judgments. Now according to the theory, the crucial factor determining the curve of judgment-time is the location of the boundaries of the regions. If the distance between the boundaries of the central category is great (the case in which judgments of equality are freely given), a large proportion of the stimuli which yield judgments of equality will fall some distance from the two boundaries. As a result, only a few of the judgments of equality will be retarded and the average time for the central category will be rather short. If, however, the distance between the boundaries is small (the case in which the judgments of equality are opposed by the attitude), a large proportion of the stimuli which yield judgments of equality will fall very near the two boundaries. Under these conditions a great many of the judgments of equality will be retarded and the average time for the central category will be rather long. It may be concluded then, in general, that the average judgment-time for any category of response depends upon the number of judgments which are retarded by conflict in relation to the number of judgments which are free from conflict. For the central category, this ratio depends upon the distance between the two boundaries of the central region.

5. The relation between the similarity of the comparative stimuli and judgment-time has long been considered a simple one. Quite generally investigators (15, 21, 18, 16) have agreed that judgment-time increases with increasing similarity

of the stimuli. Recently, however, Carter (**5, 6**) has suggested that the relationship is not so simple, that under certain limited conditions judgment-time may actually decrease with increasing similarity. What relationship does the present theory imply? In answering this question it is necessary to distinguish between experimental conditions employing two categories and those using three. The theory predicts a different relationship for the two conditions.

When two categories are employed there are, as we have seen, two regions operative in the phenomenal field with one boundary separating them. Unless there is some systematic error, such as a time-error, this boundary will coincide with the variable stimulus which is objectively equal to the standard (*i.e.*, with maximal similarity). Since the theory states that judgment-time will be maximal at this boundary and will decrease as stimuli deviate from it, the theory clearly predicts that, under these conditions, judgment-time will increase with increasing similarity and that maximal similarity will yield maximal judgment-time.

When there are three categories, however, three regions are distinguished in the phenomenal field and two boundaries separate them. Assuming that there is no systematic error, the judgments of equality will cluster symmetrically about the stimulus objectively equal to the standard and the two boundaries will lie, therefore, on either side of that stimulus. Since the theory states that judgment-times will be maximal at the boundary of a region, it must predict that the curve of judgment-time will now possess two maxima—one on each side of objective equality. Under these conditions, the relation between similarity and judgment-time will, therefore, be a complex one. As similarity decreases from objective equality to the boundary of the central category, the judgment-time will *increase*. Further decreases in similarity will lead to *decreases* in judgment-time.[9]

The crucial question for the theory is whether the relationship between similarity and judgment-time is actually found to differ for two and for three categories in the prescribed

[9] Johnson (**16**) has suggested that judgment-time is maximal at the threshold, but none of his experiments employed three categories of response.

way. With only one exception, the previous studies have employed two categories, and in accordance with the theory, they have yielded the simple direct relationship. The one study which has used three categories was conducted by Kellogg (18), and although he concluded that the simple direct relationship was exhibited by his data, a detailed analysis shows that actually our theory is corroborated in every detail.[10] In order to provide a further test of the theory, the writer (7) conducted some experiments in which the central category was made especially broad. Under these optimal conditions the complex relationship predicted by the theory was clearly obtained.[11] Thus, it is seen that the data confirm the theory under both types of experimental conditions. With the establishment of this theory, moreover, the usual statement of the relationship between similarity and judgment-time will have to be revised since this relationship, itself, depends upon the differentiation of the phenomenal field.

IV. Summary and Conclusions

The theory outlined above may be summarized as follows: (1) A conflict of forces acting upon the person lengthens reaction-time. Ordinarily such a conflict arises in decisions. (2) The act of judging, however, is a special case of this principle too, in that a conflict of forces is present when there is uncertainty of judgment. (3) This conflict arises and the judgment is retarded when the stimulus which is to be categorized falls upon the boundary of a region in the phenomenal field. (4) Since the differentiation of the phenomenal field is determined by the location of the boundaries of the regions in that field, judgment-time will depend upon the differentiation of the phenomenal field.

Four sets of data from non-judgment situations are presented in support of the statement that reaction-time is retarded by a conflict of forces. (1) The frequency of crossing in an obstruction box is reduced when two equally

[10] See Cartwright (7); especially Fig. 26.

[11] See Cartwright (7). Figs. 2, 5, and 18 are illustrative.

strong needs are opposed (**23**, pp. 71–82). (2) The latency of response is increased when two 'intentions' are opposed (**1**, **22**). (3) Reaction-time is increased when reproduction is opposed to a 'complex' (**17**). (4) There is a high correlation between decision-time and an independent measure of conflict (*i.e.*, fluctuation) in experiments upon the level-of-aspiration in manic-depressive psychoses (**20**, pp. 246–258).

Five sets of facts support the theory as applied to the act of judging. (1) Judgment-time is maximal for stimuli yielding a relative frequency of judgments of 50 per cent (**18**, **5**, **7**). (2) This relationship not only holds in traditional psychophysical experiments, but also in a wide variety of situations involving judgments (**7**). (3) The greater the number of categories which are applied to a given range of stimuli the longer will be the average judgment-time (**18**). (4) Conflicting findings as to whether or not judgments of equality take longer than judgments of difference are reconciled by showing that the average time for judgments of equality depends upon the *range* of judgments of equality (**24**, **14**, **10**, **17**, **12**, **4**, **11**, **9**, **7**). (5) The way in which judgment-time varies with the similarity of the comparative stimuli depends upon the number of categories employed in the experiment (**15**, **21**, **18**, **5**, **6**, **7**).

The demonstration that judgment-time depends upon the differentiation of the phenomenal field shows *a fortiori* the importance of differentiation itself. The concept has been proposed in studies of many different phenomena. Allport and Klüver (**2**, **19**), for example, have suggested that attitudes render stimuli equivalent and that a change of attitude will, therefore, change the differentiation of the phenomenal field. Such processes as ontogenetic development, learning, and frustration, moreover, are often held to affect the differentiation of the phenomenal field. This concept has also found application (**8**) in an analysis of the determinants of visual speed. Further investigation of these problems should be facilitated by the techniques employed in the present study.

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SOCIOLOGY AND PSYCHOLOGY IN THE PREDICTION OF BEHAVIOUR

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I. A Sociological Psychology?

Psychologists are becoming increasingly aware of the importance of sociology for the study of behaviour. There are many indications, in modern psychological literature, of a rapprochement between the two branches of investigation. One of the most striking is in the theory of motivation, where, with the decline and the instinct theory, more and more emphasis is being placed upon social and cultural factors in the development of human motives. Recent texts on general psychology and personality give similar evidence of increasing interest in the role of social groupings and organisation in the determination of behaviour. Boring, Langfeld and Weld's *Introduction to Psychology*,[1] begins by considering the 'Social Functions of the Individual.' Stagner's *Psychology of Personality* [2] includes a chapter on 'Personality and Patterns of Culture' by an anthropologist. Texts in social psychology have for many years emphasised the sociological basis of behaviour. Most striking of all is the recent text of Pressey, Janney and Kuhlen, entitled *Life: A Psychological Survey*.[3] This book introduces the student to psychology by way of an outline of the 'Conditions and Circumstances of Life,' hoping in this way to enable the student to see man embedded in the social matrix in which he lives. So instead of introductory chapters on elementary neurology we find chapters using statistical data to present a graphic outline of recent social trends, followed by a chapter

[1] E. G. Boring, H. S. Langfeld, & H. P. Weld. *Introduction to psychology.* New York: Wiley, 1939.

[2] R. Stagner, *Psychology of personality.* New York: McGraw-Hill, 1937.

[3] S. L. Pressey, J. E. Janney, & R. G. Kuhlen, *Life: a psychological survey.* New York & London: Harpers, 1939.

on the 'Invisible Environment—Culture' before we reach material which is normally included in a psychological text.

As a final indication of the trend towards a 'sociological psychology' we may cite Professor Tolman's article in this JOURNAL for 1938, in which he concludes that "the facts and laws of psychology are . . . in some part dependent upon those of sociology." [4] Surely it is highly significant that one of the leading theorists of present-day psychology should come to this conclusion! It may, perhaps, have been expected from a sociologist, but hardly from a psychologist (and incidentally one who has not even been weaned from the fold by specialising in social psychology)!

What are the reasons for this trend, and is Professor Tolman's conclusion correct? These are the problems taken up in this paper. The thesis defended is that the main reason for the rise of 'sociological psychology' is the desire to predict and control behaviour, while Professor Tolman's conclusion is true of the facts of psychology but not of its laws.

II. PSYCHOLOGY AND LIFE

All psychologists would agree that their goal is to *explain* behaviour, though there would doubtless be some disagreement over the meaning of 'behaviour.' Few psychologists would deny that they desire to *predict* behaviour, and it is not unlikely that the greater part of them desire to *control* behaviour. McDougall once wrote, "the aim of psychology is to render our knowledge of human nature more exact and more systematic, in order that we may control ourselves more wisely and influence our fellow men more effectively." [5] There is no reason to doubt that this is a fair statement of the aims of most students of human behaviour. And as evidence of this we have this century's extraordinary progress in the application of psychological knowledge to problems of living. The proof that psychological science *can* contribute to the solution of practical problems has doubtless spurred psychologists on to further efforts in this direction.

[4] E. C. Tolman, Physiology, psychology and sociology. PSYCHOL. REV., 1938, 45, 228.

[5] W. McDougall, *An outline of psychology*. London: Methuen, 1923. P. 1.

There are several factors which have operated and still operate to increase psychologists' interest in practical problems. There has always been, and doubtless always will be, the public's demand for help in problems of living, and no doubt it has been augmented by the increasing success of psychology in this respect. But it does not explain the sudden increase in psychology's practical value which occurred in the second and third decades of this century.

Nor has the application of experimental methods to behaviour been the primary cause of this spurt of progress, for although much valuable knowledge had been won in the laboratory, psychology did not receive its practical impetus from that quarter, but from outside its walls altogether.

The two factors which appear to have been primarily responsible were the development of psycho-therapy and the invention of intelligence tests. Both developments took place in the field, so to speak, and with much profit to both theory and practice. These two movements have played the major part in convincing the professional psychologist of the practical value of his science, and driven him from his pensive citadel to study actual persons in the business of living.

Here we find the first factor which helped to bridge the gap between psychology and sociology, for as soon as psychologists begin to study life as it is lived they are near to discovering how important sociological factors are in the determining of phenomena they are investigating.

But their nearness to the importance of sociological factors did not prevent their ignoring it, and it took some years for the opposition to the instinct theory of motivation to accumulate. When the storm broke and much of the all-powerful heredity of the instinct theory had been swept away, the environmental influences received more attention. And after a period of delving into the mysteries of synapse and neurone, psychologists at last have come to realise the basic importance of the social environment in the moulding of personality. That is to say, they have realised that *what* does the moulding may be important as well as *how* the moulding is done.

Another still more recent trend in psychological theory is towards the recognition that there is more to man than nervous patterns; that man is a *person*, and a person lives in a social context, a larger whole of which he forms a part.

Professor Pressey has put the three factors making for the development of a 'sociological psychology' into one sentence. In justification of the plan of *Life: A Psychological Survey*, he writes, "If psychology as distinct from physiology is concerned primarily with man considered as a social organism, such an approach would seem theoretically sound. If (as is surely the present trend of thinking) society much more than heredity is to be looked to, for explanations as to why a particular person is what he is, then such a beginning is logical. If practical readers are looking to psychology for aid in learning how to get along with other people and make their ways in the economic (and social) world, then such an approach has much practically to recommend it." [6]

III. Sociological and Psychological Facts

Professor Pressey's book will probably be received as a work on applied psychology. But this does not appear to be altogether the intention of the writer, and its significance is wider than that. Professor Pressey shows his fundamental position in the statement that "forty years ago psychology dealt with mental states, twenty years ago with reflex patterns; now increasingly it is dealing with persons . . . and therefore stressing the socio-economic and cultural environment and problems of adjustment thereto." [7]

But in this work, the first avowed treatise on 'sociological psychology,' what do we find? A description of the social environment in which the modern American lives; an account of his physical, mental and social development throughout his life span; an application of the first two sections to particular 'problems of living.' Nowhere do we find an attempt to make general statements of relationships between sociological facts and behaviour. We are told: "This is what the

[6] *Op. cit.*, p. xxix.
[7] *Ibid.*, p. xxxi.

individual will meet" and "This is what the individual will be like" and "Therefore this is the way to obtain certain results." We are not told of any general relationships holding between organism-environment complexes and behaviour. These, indeed, are taken for granted, and the book *is* a text in applied psychology, though in a broader sense than is usually implied by that phrase.

In order to control events, three things are necessary. First the relationships which hold between events must be discovered. On the basis of this knowledge the course of events may be predicted, and steps taken to procure (or prevent) it. Predicting behaviour is a necessary prologue to controlling it; discovering relationships is the necessary foundation of prediction. To control events three kinds of statement are required:

(*a*) If A, then B.

(*b*) Since A, therefore B.

(*c*) Let us then prevent (or procure) the occurrence of A, since B is undesirable (desirable).

Statements of the type (*c*) are traditionally admitted to be *applications* of types (*b*) and (*c*). With reference to the present topic, for instance, Professor Tolman writes: "It is such . . . group processes which the sociologist is interested in predicting, *and if he be an applied sociologist, of eventually learning to control*" [8] (my italics).

A statement of type (*b*) requires not only the knowledge of the statement "If A, then B," but also of A's occurrence (the minor premise). The occurrence of A may require explanation which only another science can provide, and it is just this which has led Professor Tolman astray.

Whether we choose to restrict the scope of science to statements of the type (*a*), and call both (*b*) and (*c*) applications of science, or prefer to call both (*a*) and (*b*) scientific statements, we cannot avoid recognising the essential difference between the two types of statement. Type (*a*) is a statement of relationships: *an explanation.* Type (*b*) is a

[8] *Op. cit.*, p. 232.

statement about facts; present (or past) and soon to follow. It is a prediction.

The explanation of behaviour involves relating behaviour to certain organism-environment complexes, which are predominantly social situations. The prediction of behaviour therefore involves the recognition that such situations exist. It is at this point that the psychologist must either turn sociologist or accept the results of sociology. The facts of psychology and sociology are inevitably coexistent. Indeed, as has often been pointed out, they are the same phenomena seen in different frames of reference. The same is true of all sciences. The predictions of a sociologist require the recognition of certain physical facts, and so do the predictions of a psychologist. From this point of view, the *facts* of psychology are as truly dependent upon those of physics as they are on those of sociology. The laws of behaviour discovered by psychologists under certain physical conditions may not hold under other physical conditions. In practice, of course, we have rarely to allow for this relative dependence on physical conditions, whereas the sociological conditions under which psychological laws have been discovered are subject to frequent change. The recognition of the relativity of psychological generalisations to particular sets of sociological conditions is indeed one of the major advances of psychology in the last few years. But this does not involve the reduction of psychological laws to sociological laws. Under certain sociological conditions, certain uniformities of behaviour may be discovered. Under other conditions of social structure, other uniformities may hold. The discovery of these uniformities is the primary business of psychology. Once discovered, they may be used to predict (and control) behaviour under certain observed sociological conditions. It would seem more convenient to restrict the term psychological science to the discovery of such uniformities, that is to say, to the explanation of behaviour. The prediction and control of behaviour may well be regarded as applications of psychological science.

The psychologist, then, is in no peculiar position in that his predictions force him to draw on other sciences. All scientists do this. But he is presented with a technical difficulty, the isolation of his independent variables. This he solves by the well-known methods of science, allowing for various intruding factors (such as socio-economic status in the comparison of intelligence) by means of statistical or experimental technique. Frequently the only method available to him is the observation of behaviour under different sociological conditions. Such a method enables him to estimate the influence of social environment in moulding the basic motives to action, for instance, and without it he would doubtless be comparatively helpless.

The importance of sociology for psychology lies, therefore, in the identity of the material with which the two sciences deal, namely human behaviour. Each science studies this same material from different viewpoints, and each meets the technical difficulty of isolating its variables. No scientist who wishes to predict as well as to explain can hope to do so without going beyond the boundaries of his science. Psychologists, therefore, have much to learn from studies of current social trends and the prevailing social structure, and without a knowledge of these *facts* will be unable either to predict behaviour or even to formulate explanations of behaviour. But psychological *laws* are quite independent of both the *facts* and *laws* of sociology.

The development of a 'sociological psychology' may therefore be hailed as a great and necessary advance in the study of behaviour. In the first place it makes possible prediction of behaviour on the basis of the uniformities (explanations) so far discovered. Secondly it makes possible the discovery of further uniformities, without which the scope of prediction would be severely limited. In this way it advances *application* of psychological science and helps to overcome a technical difficulty in the *explanation* of behaviour. But it should not be regarded as breaking down the logical distinction between the two sciences.

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THE MEASUREMENT OF RETENTION BY THE RELEARNING METHOD

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Since Ebbinghaus' classical study on memory the relearning method has been frequently employed in the measurement of retention of both verbal and nonverbal materials. It seems to be regarded as one of our most dependable methods for describing quantitatively the efficiency of retention over a given interval of time. One of the assumptions, however, upon which this method is based does not seem to be fulfilled by the procedure frequently employed in current laboratory work on memory.

The assumption referred to above is that the ability of the subject to learn the particular material is the same or approximately the same at the time of relearning as it was at the occasion of the original learning. Unless this assumption is made the relearning method would appear to be of little significance from the standpoint of measuring the retention over the interval. An examination of this assumption in the light of transfer data indicates pretty strongly that the ability to learn the particular material being used may be considerably different at the relearning test as compared with what it was at the original test. Furthermore, the extent to which the retention score may be modified by the condition in question probably varies with the length of the interval between learning and relearning.

Ebbinghaus may have been aware of the relevancy of the positive transfer effect in using saving scores as measures of retention as is indicated in the following quotation from his discussion of the 'Introduction of numerical measurements for memory contents': "A poem is learned by heart and then not again repeated. We will suppose that after a half year

it has been forgotten; no effort of recollection is able to call it back again into consciousness. At best only isolated fragments return. Suppose that the poem is again learned by heart. It then becomes evident that, although to all appearances totally forgotten, it still in a certain sense exists and in a way to be effective. The second learning requires noticeably less time or a noticeably smaller number of repetitions than the first. *It also requires less time or repetitions than would now be necessary to learn a similar poem of the same length.*[1] In this difference in time and number of repetitions we have evidently obtained a certain measure for that energy which a half year after the first learning still dwells in that orderly complex of ideas which make up the poem. After a shorter time we should expect to find the difference greater; after a longer time we should expect to find it less" (4, p. 8–9). The two sentences immediately preceding "In this difference . . ." in the above quotation indicate that there are likely to be two difference scores instead of one, and under some conditions the second may vanish while the first remains significantly above zero. Retention would undoubtedly be present in both procedures but not unambiguously indicated as to amount in either. After partial recognition of the transfer effect in this connection it appears to have been ignored rather generally in the consideration of the particular conditions to be fulfilled before using the saving score as a measure of retention.

Transfer studies have clearly shown that the ability to learn a given kind of material may be greatly improved by earlier training on material of the same class. In general, the greater the similarity between the materials being learned the greater the positive transfer effect. Even the mastery of one maze, one rational learning problem, list of nonsense syllables, etc., has been reported to have increased the efficiency with which the second was learned by an amount from near zero up to fifty or sixty per cent. If the second test is one of relearning, it is very probable also that a con-

[1] Italics not in the original.

siderable saving will occur in the mastery of that part of the problem which has been forgotten over the interval as compared with the effort required in the initial mastery of it. When such a result is present and the extent to which the 'retention' score is a function of the amount of transfer effect is not, of course, indicated by the saving score itself.

Some recent experimental studies on transfer and retention show that, at least under some conditions, the two do not vary with time in the same manner. While the ability to relearn memory material has usually been found to vary in a negatively accelerated manner with the passage of time, the amount of transfer from one memory task (nonsense syllable problem) to a second was found to be independent of the length of time, up to 28 days, elapsing before the learning of the second task (1). The same general result was found when rational learning problems constituted the material and both retention and transfer were measured after an interval of 90 days (2). Another result, of interest in a consideration of the adequacy of the relearning method as a measure of retention, is that found in a comparison of retention and transfer in maze learning with white rats as subjects (3). When an interval of 120 days elapsed before the second maze was learned, the percentage saving from transfer was approximately the same as occurred under the zero time-interval condition. The saving from retention varied with time in a negatively accelerated manner. The superiority of the saving from retention over that from transfer disappeared completely during this interval. In other words, the retention of the first maze decreased to a level where, in terms of relearning measures, it was functionally equivalent to the transfer effect from training on a similar maze equally remote in the animal's past.

Presumably a transfer effect in the learning of a second similar task is dependent to some extent upon some retention of the effects of the previous training. A marked amount of forgetting may take place without, however, modifying significantly the transfer effect which this learning has upon the subsequent mastery of a similar problem selected by the

experimenter. Such independent variability was found in the study with rational learning problems where saving scores, based on relearning measures, declined markedly with time without a corresponding change in the positive transfer effect.

It is usually regarded that both transfer and retention, as these concepts are used in experimental studies, are involved in the original learning of a problem and condition to a great extent the efficiency shown by the subject at the time. It is probable that both are involved in the later relearning of the problem and that *their relative contribution to the efficiency of the subject in the relearning test varies with the passage of time.* An expected large drop in the efficiency of retention over a rather long interval may be partially offset when saving scores are used by a significant amount of positive transfer effect in the relearning of the forgotten material, since the transfer effect may be independent of fairly long intervals of time. The possible interpretation of the change in the relative position of the relearning method with respect to other methods of measuring retention that occurs with time, as found by Luh (6), in terms of an increasing transfer effect in relearning has been mentioned earlier (1).

The relative contribution of retention and transfer to the saving score obtained by the relearning method may possibly be influenced by various experimental conditions or variables. In a study of the relative effect of a cerebral lesion of a given size and locus upon learning, transfer, and retention, Ericksen employed a procedure in which some of the animals were operated on before learning, some after learning but before the relearning test, and some after the first learning but before the test for transfer of training. According to the result, "the lesion exerted the greatest retardation upon retention and the least upon learning, with transfer occupying an intermediate position" (5, p. 388). With respect to our present question, it is probable that the relative effect he found would not be entirely a function of the particular problem on which transfer was studied but that a differential

effect upon transfer and retention might occur as these operate in a relearning test.

Maier's study of the influence of cortical injuries on the process of 'direction' is of interest in this connection. After two groups of rats were trained on a simple reasoning problem, one received additional training on a more complex problem for the purpose of establishing the direction or set being studied. The two groups were then operated on, and tested on both problems after brain injury. The data indicated that a technique in problem solving, once established, may survive brain injury and be of aid to the animal but cannot be developed after marked brain injury. Direction is conceived as an integrating function. It is stated that "Since brain injuries affect this type of process in a different manner from the way they affect memories, we regard the *direction* process as different in kind from the elements it integrates" (7, p. 79). Such a direction process, of aid in solving similar problems, might be of definite aid also in solving again the original problem when as a result of brain injury the solution is not remembered.

There is then not only the likelihood of a transfer effect in the relearning of that part of the original problem which has been forgotten over the interval, but also the possibility than an experimental variable whose influence upon retention is being studied may exert an effect upon the saving score which is not alone a function of its influence upon retention. There would seem to be such a possibility whether the variable being studied is introduced before or following initial learning. If the subject's ability to learn the particular material has been significantly modified by the variable under study, as *e.g.*, drug, brain injury, other learning, etc., then the relearning data are somewhat ambiguous as a measure of the influence of this variable upon retention.

It is quite possible that subjects who are experienced in learning a given kind of material may show a smaller saving score, measured by the relearning method, than naive subjects under the same conditions without the difference being

produced by poorer retention over the interval. The ability to master a given kind of material probably improves more from the first to the second such problem, and presumably also to a relearning of the first if that is what the second situation calls for, than it does, say, from the twentieth to the twenty-first. For example, Ward (8) found a rather marked improvement in the learning of lists of nonsense syllables through the first six lists, and to a lesser degree thereafter, of an experimental series involving sixteen separate lists. In his study, the learning was continued through one perfect trial, and the subjects had had four practice learning sessions with this kind of material prior to the experiment proper. After considerable experience in learning a given kind of material, the subject's ability to learn this material is more nearly the same or approximately the same in the relearning situation as in the original learning of that problem. Consequently, under these conditions, the saving score would seem to be a more satisfactory measure of retention of the problem for the interval, instead of measuring retention plus an unknown amount of improvement in the ability to master such material over the relatively low level exhibited in the initial learning.

We may conclude, therefore, that there is a limitation of the relearning method as frequently used. This conclusion, however, does not mean to imply that the method should be regarded as necessarily less useful or significant in experimental work on memory but rather that greater care should be exercised with regard to the conditions under which relearning scores are obtained and in the interpretation of the saving score as a measure of retention.

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